FISCAL YEAR 1984
DEPARTMENT OF ENERGY AUTHORIZATION
(Magnetic Fusion Energy)

HEARINGS

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

NINETY-EIGHTH CONGRESS

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1984 DEPARTMENT OF ENERGY AUTHORIZATION

WEDNESDAY, MARCH 16, 1983

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Research and Production,
Washington, D.C.

The subcommittee met, pursuant to notice, at 2:01 p.m., in room 2325, Rayburn House Office Building, Hon. Marilyn Lloyd Bouquard (chairman of the subcommittee) presiding.

Present: Representatives Bouquard, Hall, Walker, Chandler, and Lowery.

Staff present: John V. Dugan, staff director, and William Harvey, minority energy coordinator.

Mrs. Bouquard. The subcommitee will come to order.

We are in the unusual position today of hearing from the outside witnesses on the magnetic fusion energy program before the Department of Energy testifies. I felt that our tight hearing schedule could not be shifted to accommodate the Department because we had so many witnesses who had already made plans to be in Washington to testify today on this program.

I am very encouraged by both the quality and the quantity of our witnesses today. I must say it is a strong testament to the health of the fusion program that we do have such enthusiastic utility and industrial spokesmen, as well as three old friends who have been strongly supportive of the program for many years at national laboratories and on the committee.

In general, I believe that our first panel is strong evidence of the need to encourage increasing industrial involvement and responsibility in the development of this exciting technology.

Our second panel of spokesmen from the three major university centers in fusion is, similarly, strong evidence for the high quality of that effort.

I am quite concerned about the national laboratory/ university cooperation in magnetic fusion and hope that today we can learn about how to improve the interaction. Of these three university witnesses one's name is associated with an enrichment technology already quite familiar to the subcommittee, and another has been associated with fusion from its early days and now is at a major university in one of this country's growing high-technology centers.

I am looking forward to hearing from all eight witnesses so we can have a solid basis for constructive subcommittee action. I do regret that at some point I will have to turn the Chair over to Mr.

Ralph Hall because of the press of hearings in my other subcommittee's areas.

Our first panel today consists of Mr. Kenneth Matson; Dr. Harold Agnew; Dr. Stephen Dean; Mr. Mike McCormack, former chairman of this committee; and Dr. Allan Mense.

Mr. Matson, would you please proceed?

STATEMENT OF KENNETH L. MATSON, VICE PRESIDENT, PUBLIC SERVICE ELECTRIC GAS RESEARCH CORP., AND CHAIRMAN, POLICY PANEL OF THE ATOMIC INDUSTRIAL FORUM'S COMMITTEE ON FUSION

Mr. **Matson**. Thank you, Madam Chairman.

My name is Kenneth Matson. I am vice president of PSE&G Research Corp., which is a wholly owned subsidiary of Public Service Electric & Gas Co., headquartered in Newark, N.J. I am also a member of the Atomic Industrial Forum's Committee on Fusion and chairman of the committee's policy panel. It is in that capacity that I am appearing here today.

The Atomic Industrial Forum is a trade associaton with about 550 member organizations. More than 100 of the member organizations are outside the United States—in Europe, the Far East, and the Americas. All of the organizations share a dedication to the peaceful use of nuclear energy for mankind's needs in energy, medicine, and industrial development. The member organizations include electric utilities, architect-engineers, equipment manufacturers, mining and milling companies, labor unions, laboratories, financial institutions, and universities.

On behalf of the AIF Committee on Fusion, I want to thank you for this opportunity to present the committee's views on DOE's fusion program and the proposed program budget for fiscal year 1984.

The AIF's Committee on Fusion has followed the development of fusion power since 1975 and has testified before this and other committees of the Congress.

In its report, "Fusion Energy at the Crossroads: Role of the Private Sector," dated December 31, 1979, the AIF committee called for the early and active participation of industrial and utility companies in the fusion program. A copy of this report was entered into the record of hearings of your subcommittee on July 28, 1980.

Since these industrial and utility companies will have the responsibility for designing, constructing, operating, and improving fusion energy plants when they are built, their extensive involvement is essential to fusion power development. And because of the complexity of fusion science and technology in which the industry has a limited role and the consequential long learning periods, it would be beneficial if industry's experience and talents could be focused now on the development of a fusion engineering data base.

The AIF committee found that the state of fusion development warrants funding at a level to support the 1990 operation of a fusion engineering reactor, and that the establishment of a center for fusion engineering, a CFE, is key to the success of such a program.

The CFE, it noted, would provide the best institutional arrangement for carrying forward the requisite engineering effort under industrial management. Since that time, the AIF committee has reluctantly acknowledged that overall Federal budget constraints will delay achieving this goal. Given this reality, the committee, in its most recent review of fusion policy, proposes the following program considerations:

Key to demonstrating in a reasonable time a controlled plasma and to proceeding with engineering development is a strategy to upgrade the Tokamak fusion test reactor, [TFTR], and the Mirror fusion test facility [MFTF].

Expanding DOE's program for coordinated effort between the cognizant national laboratories, universities, and industry in pursuing this strategy can strengthen the program. The laboratories and universities can benefit from industry's experience and capabilities in design, engineering development, manufacture and operation of large, complex industrial equipment, components, and facilities. Industry, on the other hand, can benefit from the laboratories' and universities' more detailed understanding of plasma physics and related technology.

Fusion technology can be developed with maximum assistance from industry by expanding the efforts of industrial contractors in developing the TFTR and MFTF upgrade technology packages. Expansion of efforts should include packages for the management and engineering of subsystems, including their engineering design, development, and manufacture.

To achieve even these limited program objectives, increasing budget support, however small overall Federal budget pressures dictate the increases will be, is required. The $20 million increase in the proposed fiscal year 1984 budget is considered a minimum to meet these objectives.

In addition, the committee believes that the following steps should be taken now to develop engineering data to support an upgrade program and to provide the basis for a meaningful engineering data base for the next integrated fusion test facility:

Expand the program for technology/engineering testing in existing facilities. Industry has available a wide range of testing facilities, particularly those previously developed for nuclear fission and aerospace technologies, which can be effectively adapted to augment the data needs of the fusion program.

Earmark specific Government funding in the fiscal year 1984 budget for development in industry of an engineering development plan. A fund of about $1 million would provide for the parallel development in industry of a number of plans for developing a fusion engineering data base and would complement DOE's new fusion technology development plan. Such a step would marshall the broad engineering expertise of industry and assure development of an adequate data base.

As fusion technology evolves, set aside annual funding for a number of industrial initiatives in cooperation with selected national laboratories and universities to develop needed engineering data. This would promote cooperation between industry and the national laboratories or universities and begin engineering tasks these groups believe are important to the demonstration of engineering feasibility. The details of such an effort could be spelled out in a jointly developed work/expenditure statement.

In your letter, Madam Chairman, you asked what funding levels and what specific program activities not contained in the 1984 fiscal year budget request would be necessary to maintain the aggressive program pace envisioned in the Magnetic Fusion Engineering Act of 1980.

The AIF Committee believes that progress in magnetic fusion development is currently limited by available funds rather than technical barriers and that it would take the 25-percent increase that Congress found would be required in the 1980 legislation to move the program ahead at a pace envisioned in the act. As I noted earlier in my testimony, the AIF Committee has found that the establishment of a center for fusion engineering, a CFE, is key to the success of the act's aggressive program.

Your letter also asked the desirability of amending the Magnetic Fusion Engineering Act to reflect the lengthened schedule for fusion energy development proposed by the current administration. We believe the act represents a policy for the fusion program that is realistic and achievable if funded adequately. There appears to be no reason for changing the basic policy established by the Congress. The budget constraints can, and perhaps should, be accepted until the state of the economy improves. It should not, however, be used as the rationale for changing the basic thrust and direction of the fusion program.

The task of developing fusion power to make a major contribution to the Nation's future energy needs will not be an easy one. And yet, the scientific evidence now in hand that fusion has the potential for providing a virtually inexhaustible source of energy for future generations warrants our best efforts.

This concludes my prepared remarks. Thank you. I would be pleased to answer any questions.

[The prepared statement of Mr. Matson follows:]

ATOMIC INDUSTRIAL FORUM
COMMITTEE ON FUSION
TESTIMONY BY KENNETH L. MATSON
BEFORE THE SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTAIVES
MARCH 16, 1983

MY NAME IS KENNETH L. MATSON. I AM VICE PRESIDENT OF PSE&G RESEARCH CORPORATION, A WHOLLY-OWNED SUBSIDIARY OF PUBLIC SERVICE ELECTRIC AND GAS COMPANY, HEADQUARTERED IN NEWARK, NEW JERSEY. I AM ALSO A MEMBER OF THE ATOMIC INDUSTRIAL FORUM'S COMMITTEE ON FUSION AND CHAIRMAN OF THE COMMITTEE'S POLICY PANEL. IT IS IN THAT CAPACITY THAT I AM APPEARING HERE TODAY.

THE ATOMIC INDUSTRIAL FORUM IS A TRADE ASSOCIATION WITH ABOUT 550 MEMBER ORGANIZATIONS. MORE THAN 100 OF THE MEMBER ORGANIZATIONS ARE OUTSIDE THE U.S.--IN EUROPE, THE FAR EAST AND THE AMERICAS. ALL OF THE ORGANIZATIONS SHARE A DEDICATION TO THE PEACEFUL USE OF NUCLEAR ENERGY FOR MANKIND'S NEEDS IN ENERGY, MEDICINE, AND INDUSTRIAL DEVELOPMENT. THE MEMBER ORGANIZATIONS INCLUDE ELECTRIC UTILITIES, ARCHITECT-ENGINEERS, EQUIPMENT MANUFACTURERS, MINING AND MILLING COMPANIES, LABOR UNIONS, LABORATORIES, FINANCIAL INSTITUTIONS AND UNIVERSITIES.

ON BEHALF OF THE AIF COMMITTEE ON FUSION I WANT TO THANK YOU FOR THIS OPPORTUNITY TO PRESENT THE COMMITTEE'S VIEWS ON DOE'S FUSION PROGRAM AND THE PROPOSED PROGRAM BUDGET FOR FISCAL YEAR 1984.

THE AIF'S COMMITTEE ON FUSION HAS FOLLOWED THE DEVELOPMENT OF FUSION POWER SINCE 1975 AND HAS TESTIFIED BEFORE THIS AND OTHER COMMITTEES OF THE CONGRESS.

IN ITS REPORT, "FUSION ENERGY AT THE CROSSROADS: ROLE OF THE PRIVATE SECTOR," DATED DECEMBER 31, 1979, THE AIF COMMITTEE CALLED FOR THE EARLY AND ACTIVE PARTICIPATION OF INDUSTRIAL AND UTILITY COMPANIES IN THE FUSION PROGRAM. A COPY OF THIS REPORT WAS ENTERED INTO THE RECORD OF HEARINGS OF YOUR SUBCOMMITTEE ON JULY 28, 1980. SINCE THESE INDUSTRIAL AND UTILITY COMPANIES WILL HAVE THE RESPONSIBILITY FOR DESIGNING, CONSTRUCTING, OPERATING AND IMPROVING FUSION ENERGY PLANTS WHEN THEY ARE BUILT, THEIR EXTENSIVE INVOLVEMENT IS ESSENTIAL TO FUSION POWER DEVELOPMENT. AND BECAUSE OF THE COMPLEXITY OF FUSION SCIENCE AND TECHNOLOGY IN WHICH THE INDUSTRY HAS A LIMITED ROLE AND THE CONSEQUENTIAL LONG LEARNING PERIODS, IT WOULD BE BENEFICIAL IF INDUSTRY'S EXPERIENCE AND TALENTS COULD BE FOCUSED NOW ON THE DEVELOPMENT OF A FUSION ENGINEERING DATA BASE.

THE AIF COMMITTEE FOUND THAT THE STATE OF FUSION DEVELOPMENT WARRANTS FUNDING AT A LEVEL TO SUPPORT THE 1990 OPERATION OF A FUSION ENGINEERING REACTOR, AND THAT THE ESTABLISHMENT OF A CENTER FOR FUSION ENGINEERING (CFE) IS KEY TO THE SUCCESS OF SUCH A PROGRAM. THE CFE, IT NOTED, WOULD PROVIDE THE BEST INSTITUTIONAL ARRANGEMENT FOR CARRYING FORWARD THE REQUISITE ENGINEERING EFFORT UNDER INDUSTRIAL MANAGEMENT. SINCE THAT TIME, THE AIF COMMITTEE HAS RELUCTANTLY ACKNOWLEDGED THAT OVERALL FEDERAL BUDGET CONSTRAINTS WILL DELAY ACHIEVING THIS GOAL. GIVEN THIS REALITY, THE COMMITTEE, IN ITS MOST RECENT REVIEW OF FUSION POLICY, PROPOSES THE FOLLOWING PROGRAM CONSIDERATIONS:

- KEY TO DEMONSTRATING IN A REASONABLE TIME A CONTROLLED PLASMA AND TO PROCEEDING WITH ENGINEERING DEVELOPMENT IS A STRATEGY TO UPGRADE THE TOKAMAK FUSION TEST REACTOR (TFTR) AND THE MIRROR FUSION TEST FACILITY (MFTF).
- EXPANDING DOE'S PROGRAM FOR COORDINATED EFFORT BETWEEN THE COGNIZANT NATIONAL LABORATORIES, UNIVERSITIES AND INDUSTRY IN PURSUING THIS STRATEGY CAN STRENGTHEN THE PROGRAM. THE LABORATORIES AND UNIVERSITIES CAN BENEFIT FROM INDUSTRY'S EXPERIENCE AND CAPABILITIES IN DESIGN, ENGINEERING DEVELOPMENT, MANUFACTURE AND OPERATION OF LARGE, COMPLEX INDUSTRIAL EQUIPMENT, COMPONENTS AND FACILITIES. INDUSTRY, ON THE OTHER HAND, CAN BENEFIT FROM THE LABORATORIES' AND UNIVERSITIES' MORE DETAILED UNDERSTANDING OF PLASMA PHYSICS AND RELATED TECHNOLOGY.
- FUSION TECHNOLOGY CAN BE DEVELOPED WITH MAXIMUM ASSISTANCE FROM INDUSTRY BY EXPANDING THE EFFORTS OF INDUSTRIAL CONTRACTORS IN DEVELOPING TFTR AND MFTF UPGRADE TECHNOLOGY PACKAGES. EXPANSION OF EFFORTS SHOULD INCLUDE PACKAGES FOR THE MANAGEMENT AND ENGINEERING OF SUB-SYSTEMS, INCLUDING THEIR ENGINEERING DESIGN, DEVELOPMENT, AND MANUACTURE.

TO ACHIEVE EVEN THESE LIMITED PROGRAM OBJECTIVES, INCREASING BUDGET SUPPORT, HOWEVER SMALL OVERALL FEDERAL BUDGET PRESSURES DICTATE THE INCREASES WILL BE, IS REQUIRED. THE $20 MILLION INCREASE IN THE PROPOSED FY 84 BUDGET IS CONSIDERED A MINIMUM TO MEET THESE OBJECTIVES.

IN ADDITION, THE COMMITTEE BELIEVES THAT THE FOLLOWING STEPS SHOULD BE TAKEN NOW TO DEVELOP ENGINEERING DATA TO SUPPORT AN UPGRADE PROGRAM AND TO PROVIDE THE BASIS FOR A MEANINGFUL ENGINEERING DATA BASE FOR THE NEXT INTEGRATED FUSION TEST FACILITY:

- EXPAND THE PROGRAM FOR TECHNOLOGY/ENGINEERING TESTING IN EXISTING FACILITIES. INDUSTRY HAS AVAILABLE A WIDE RANGE OF TESTING FACILITIES, PARTICULARLY THOSE PREVIOUSLY DEVELOPED FOR NUCLEAR FISSION AND AEROSPACE TECHNOLOGIES, WHICH CAN BE EFFECTIVELY ADAPTED TO AUGMENT THE DATA NEEDS OF THE FUSION PROGRAM.
- EARMARK SPECIFIC GOVERNMENT FUNDING IN THE FY 84 BUDGET FOR DEVELOPMENT IN INDUSTRY OF AN ENGINEERING DEVELOPMENT PLAN. A FUND OF ABOUT $1 MILLION WOULD PROVIDE FOR THE PARALLEL DEVELOPMENT IN INDUSTRY OF A NUMBER OF PLANS FOR DEVELOPING A FUSION ENGINEERING DATA BASE AND WOULD COMPLEMENT DOE'S NEW FUSION TECHNOLOGY DEVELOPMENT PLAN. SUCH A STEP WOULD MARSHALL THE BROAD ENGINEERING EXPERTISE OF INDUSTRY AND ASSURE DEVELOPMENT OF AN ADEQUATE DATA BASE.
- AS FUSION TECHNOLOGY EVOLVES, SET ASIDE ANNUAL FUNDING FOR A NUMBER OF INDUSTRIAL INITIATIVES IN COOPERATION WITH SELECTED NATIONAL LABORATORIES AND UNIVERSITIES TO DEVELOP NEEDED ENGINEERING DATA. THIS WOULD PROMOTE COOPERATION BETWEEN INDUSTRY AND THE NATIONAL LABORATORIES

OR UNIVERSITIES AND BEGIN ENGINEERING TASKS THESE GROUPS BELIEVE ARE IMPORTANT TO THE DEMONSTRATION OF ENGINEERING FEASIBLITY. THE DETAILS OF SUCH AN EFFORT COULD BE SPELLED OUT IN A JOINTLY DEVELOPED WORK/EXPENDITURE STATEMENT.

IN YOUR LETTER MADAM CHAIRMAN YOU ASKED WHAT FUNDING LEVELS AND WHAT SPECIFIC PROGRAM ACTIVITIES NOT CONTAINED IN THE 1984 FISCAL YEAR BUDGET REQUEST WOULD BE NECESSARY TO MAINTAIN THE AGGRESSIVE PROGRAM PACE ENVISIONED IN THE MAGNETIC FUSION ENGINEERING ACT OF 1980. THE AIF COMMITTEE BELIEVES THAT PROGRESS IN MAGNETIC FUSION DEVELOPMENT IS CURRENTLY LIMITED BY AVAILABLE FUNDS RATHER THAN TECHNICAL BARRIERS AND THAT IT WOULD TAKE THE 25% INCREASE THAT CONGRESS FOUND WOULD BE REQUIRED IN THE 1980 LEGISLATION TO MOVE THE PROGRAM AHEAD AT A PACE ENVISIONED IN THE ACT. AS I NOTED EARLIER IN MY TESTIMONY, THE AIF COMMITTEE HAS FOUND THAT THE ESTABLISHMENT OF A CENTER FOR FUSION ENGINEERING (CFE) IS KEY TO THE SUCCESS OF THE ACT'S AGGRESSIVE PROGRAM.

YOUR LETTER ALSO ASKED THE DESIRABILITY OF AMENDING THE MAGNETIC FUSION ENGINEERING ACT TO REFLECT THE LENGTHENED SCHEDULE FOR FUSION ENERGY DEVELOPMENT PROPOSED BY THE CURRENT ADMINISTRATION. WE BELIEVE THE ACT REPRESENTS A POLICY FOR THE FUSION PROGRAM THAT IS REALISTIC AND ACHIEVABLE IF FUNDED ADEQUATELY. THERE APPEARS TO BE NO REASON FOR CHANGING THE BASIC POLICY ESTABLISHED BY THE CONGRESS. THE BUDGET CONSTRAINTS CAN AND PERHAPS SHOULD BE ACCEPTED UNTIL THE STATE OF THE ECONOMY IMPROVES. IT SHOULD

NOT, HOWEVER, BE USED AS THE RATIONALE FOR CHANGING THE BASIC THRUST AND DIRECTION OF THE FUSION PROGRAM.

THE TASK OF DEVELOPING FUSION POWER TO MAKE A MAJOR CONTRIBUTION TO THE NATION'S FUTURE ENERGY NEEDS WILL NOT BE AN EASY ONE. AND YET, THE SCIENTIFIC EVIDENCE NOW IN HAND THAT FUSION HAS THE POTENTIAL FOR PROVIDING A VIRTUALLY INEXHAUSTIBLE SOURCE OF ENERGY FOR FUTURE GENERATIONS WARRANTS OUR BEST EFFORTS.

THIS CONCLUDES MY PREPARED REMARKS. I WOULD BE PLEASED TO ANSWER ANY QUESTIONS THE SUBCOMMITTEE MEMBERS MAY HAVE.

Mrs. **Bouquard**. Thank you very much, Mr. Matson, for your fine testimony.

Dr. Agnew, will you proceed with your testimony, please?

STATEMENT OF DR. HAROLD AGNEW, PRESIDENT, GA TECHNOLOGIES, INC.

Dr. **Agnew**. Thank you, Madam Chairman, very much for allowing me to appear before you. To supplement my enthusiasm, I have with me Tihiro Ohkawa. I wanted to bring the "brains" in case there is some particular question that you or members of your staff have.

Mrs. **Bouquard**. We are happy to have both of you here.

Dr. **Agnew**. What I would like to do is summarize my remarks and ask your permission that my full testimony be submitted for the record.

Mrs. **Bouquard**. Your full testimony will be made part of the record following your oral presentation. You may proceed as you wish, Dr. Agnew.

Dr. **Agnew**. Thank you very much.

I believe a national commitment to magnetic fusion is warranted by its tremendous promise and its continued rapid pace of scientific and technological advancement. I feel that the Tokamak program, in particular, is poised for a major development. I wholeheartedly support the 1984 Presidential budget but urge the addition of two modest initiatives that I wish to mention later on.

I sincerely believe that the 1984 Presidential budget keeps the program on track for providing the Nation with this vital energy option on an acceptable time scale.

The past year has been extremely productive for the magnetic fusion effort. I believe that the preeminent scientific development was the demonstration of a record of about 4.6 percent plasma beta on Doublet III, which is the large tokamak at GA. The values obtained augur well for the ability to produce desirable power densities in tokamak reactors, a point which was in question some years ago.

Engineering and technological progress has kept pace with advances on the scientific front. A prime example is the successful implementation of a state-of-the-art 60 GHz electron cyclotron heating system on Doublet III. We have also succeeded in injecting over 5 megawatts of neutral beam power into a plasma in Doublet III.

In the community we consider the most important engineering milestone was the impressive, on-time startup of the Tokamak Fusion Test Reactor at Princeton. This accomplishment is evidence of how far the program has progressed toward readiness to tackle a reactor-scale project.

These advances, taken together with other funded program elements which are still in the construction stage, are sufficient to propel the magnetic fusion program forward in the near term.

The fiscal year 1984 budget, although very tight, does provide adequate funds—and I think in an appropriate mix—to sustain this momentum in the near term. However, the absence of major new initiatives jeopardizes this progress over the longer term. While commitment to a first generation reactor is not possible in the present budgetary climate, the program should take the steps necessary to prepare for this eventuality in order to minimize slippage of the time scale for the ultimate commercialization objective.

We recommend two cost-effective steps that require modest increments to the fiscal year 1984 Presidential budget. These are:

First, the commitment to develop a conceptual design of a tokamak fusion core demonstration device, an ignited tokamak that could become operational in the early 1990's. Tomorrow Dr. Davidson will testify before you, and he will convey the enthusiasm of the Magnetic Fusion Advisory Committee for pursing this initiative. This activity would require an additional $5 million in fiscal 1984.

Second, I would suggest the establishment of a national ion cyclotron heating engineering program to manage the engineering of ion cyclotron heating hardware. This is today regarded as the most promising method for heating tokamak plasmas. The systems developed would be used to push TFTR and Big Dee at GA to their performance limits on a time scale commensurate with providing near-term guidance to reactor design. Later, the identical hardware could be used on a real reactor. In fiscal year 1984 this program will require an additional $10 million in budget authority and $5 million in budget obligation.

The latter item, I believe, should be contracted to private industry while the former should be a community effort under the Princeton Plasma Physics Laboratory.

I believe that when these two initiatives are undertaken the tokamak community will have the program elements in place to support commitment to a tokamak reactor. One further investigation is also warranted in the near term: an evaluation of the options available for gaining experience with longer pulse operation. This would require the establshment of a toroidal program office in conjunction with the selection of Princeton as lead laboratory for the toroidal program and would provide an effective mechanism for carrying out these and other program planning functions.

The incorporation of the Big Dee at GA into the program, which your committee has supported, makes us confident that we will provide a progressively increasing role in determining the form tokamak reactors will take. We are equally confident that, with your support, the magnetic fusion program as a whole will continue its steady progress toward providing the Nation with an unique inexhaustible energy option.

Again, I wish to thank you and your colleagues for allowing us to appear before you and for your continued support. I will be happy to answer questions later on. Thank you.

[The prepared statement of Dr. Agnew follows:]

Testimony before the House Science
and Technology Subcommittee
on the
FY 1984 Magnetic Fusion Energy Budget

by

Dr. Harold Agnew, President
GA Technologies Inc.

March 16, 1983

Madam Chairman; thank you for the opportunity to testify before your committee on the FY 1984 magnetic fusion budget. I am accompanied today by Dr. Tihiro Ohkawa, GA's Vice President in charge of fusion.

A reinforced national commitment to magnetic fusion is warranted by its vast promise and by the accelerating pace of scientific and technological advancement. I feel that the tokamak program in particular is poised for a major reactor-like step. With the addition of two modest initiatives that I shall describe for you, the FY 1984 Presidential budget keeps the program on track for providing the nation with this vital energy option on an acceptable time scale.

In my testimony, I shall briefly recount the GA fusion program history, indicate the most far-reaching developments of the past year, present the request for additional funds, and conclude with a commentary on the relationship between the U.S. program and that of other nations.

In many respects, the chronology of GA's involvement in the fusion program has mirrored on a modest scale the program as a whole. We began with small experiments designed to establish the basic principles of fusion physics; indeed, some of these experiments were instrumental in providing direction to the national effort. As our experimental program progressed to larger scale devices with greater capabilities, we initiated engineering testing and component development activities, all the while vigorously participating in design studies to explore the reactor embodiment of specific fusion concepts. The principal thrust of the GA program at present is Doublet III, a large tokamak which has been a landmark in international cooperative fusion research and which continues to provide much of the design base for tokamak reactors. While we anticipate continuing to make meaningful contributions to the further enrichment of the physics base for the fusion program, we now also stand poised to mobilize our resources in support of the coming engineering phase.

The fusion program at GA has been supported primarily by the Department of Energy, but it has also received support from our parent company (Gulf Oil), from other industrial concerns (most notably Phillips Petroleum), from EPRI, and directly from individual utilities. This reflects our unique role as participants in the forefront of fusion research while at the same time retaining the perspective upon commercialization associated with our industrial nature.

The past year has been an especially productive one for the GA fusion effort and for Doublet III in particular. The preeminent development was the demonstration on Doublet III of record values of plasma beta. The peak value obtained —4.5% —represents a factor of two improvement over predecessor experiments and augurs well for the ability to produce desirable power densities in tokamak reactors. Because the question of achievable power density has long been the principal critical issue for the tokamak program, this demonstration has been received with great enthusiasm among fusion researchers.

Research on Doublet III during the past year has served to confirm the advantages of non-circular cross section plasmas initially conjectured by Tihiro Ohkawa. These advantages are evidenced through accessibility to the high beta regime and through the demonstrated improvement in confinement owing to higher plasma currents and greater plasma elongation. The configurational flexibility inherent in Doublet III has also proven crucial in another series of experiments that has focused upon the improved confinement observed in discharges in which the plasma edge is defined by a magnetic limiter rather than by a physical limiter.

These scientific successes at GA have been supplemented by advances made at other laboratories. Indeed the richness of the tokamak program has been a major factor in its impressive rate of progress. We urge the Committee to retain this vital feature of the program and to continue to support vigorous toroidal confinement research at MIT, ORNL, PPPL, and GA.

Engineering and technological progress has kept pace with advances on the scientific front. A prime example is the successful implementation of a state-of-the-art 60 GHz electron cyclotron heating system on Doublet III. We have also succeeded in injecting more than 5 MW of neutral beam power into a Doublet III plasma. Program-wide, the most important engineering milestone was the impressive on-time start-up of the Tokamak Fusion Test Reactor at Princeton. This accomplishment is a tribute to the personal dedication of many extraordinary individuals and at the same time is evidence of how far the program has progressed toward readiness to tackle a reactor scale project.

These recent advances, taken together with other funded program elements which are still in the construction stage, are sufficient to propel the magnetic fusion program forward in the near term. In the mid-80s, the two principal vehicles in the tokamak program for demonstrating major progress toward reactor conditions will be TFTR and DIII-D (or Big Dee). The events of the past year have served to confirm the basic design decisions underlying these devices so there is every reason to expect that they will achieve or exceed their original objectives.

The FY 1984 budget, although very tight, does provide adequate funds (and in an appropriate mix) to sustain this momentum in the near term. However, the absence of major new initiatives jeopardizes this progress over the longer term. While commitment to a first generation reactor is not possible in the present budgetary climate, the program should take the steps necessary to prepare for this eventuality in order to minimize slippage of the time scale for the ultimate commercialization objective. We recommend two cost effective steps that require modest increments to the FY 1984 Presidential budget. These are:

1) The commitment to develop a conceptual design of a Tokamak Fusion Core Demonstration device, an ignited tokamak that could become operational in the early 1990s. Dr. Davidson's testimony before you tomorrow will convey the enthusiasm of the Magnetic Fusion Advisory Committee for pursuing this initiative. This activity requries an additional $6M in FY 1984.

2) The establishment of a National Ion Cyclotron Heating Engineering Program to manage the engineering of ion cyclotron heating hardware, regarded as the most promising method for heating tokamak plasmas. The systems developed would be used to push TFTR and Big Dee to their performance limits on a time scale commensurate with providing near term guidance to reactor design. Later, the identical hardware could be used on a reactor. In FY 1984, this program will require an additional $10M in BA and $5M in BO.

The latter item should be contracted to industry while the former should be a community effort under the direction of PPPL.

When these two initiatives are undertaken, the tokamak community will have the program elements in place to support commitment to a reactor tokamak. One further investigation is also warranted in the near term: an evaluation of the options available for gaining experience with longer pulse operation.

The establishment of a Toroidal Program Office in conjunction with the selection of PPPL as lead laboratory for the toroidal program provides an effective mechanism for carrying out these and other program planning functions.

This strengthening of the tokamak program is important for the U.S. in retaining supremacy in the worldwide effort to harness fusion power. Another key to enhancing our competitive position is participation in carefully structured international cooperative ventures. Such activities will have even greater appeal as the resources required for new projects increase. Our experience on Doublet III, which is now 3 1/2 years into a 5 year cooperative program with the Japan Atomic Energy Research Institute, has been very favorable. The Japanese have gained valuable experience on the operation of an advanced tokamak facility while our program has benefitted greatly from major hardware additions funded by Japan. These hardware additions are vitally important to our long range experimental plan.

The incorporation of the Big Dee into the program, which your Committee has wisely supported, makes us confident that we will provide a progressively increasing role in determining the form tokamak reactors will take. We are equally confident that, with your support, the magnetic fusion program as a whole will continue its steady progress toward providing the nation with an attractive, inexhaustible energy option.

Thank you for permitting me to appear before you and, of course, thank you for your continued support of our nation's magnetic fusion program.

Mrs. **Bouquard**. Thank you, Dr. Agnew. We are always happy to have you and appreciate your contribution.

Dr. Dean, please proceed. We have your testimony. You may proceed as you wish.

STATEMENT OF DR. STEPHEN O. DEAN, PRESIDENT, FUSION POWER ASSOCIATES

Dr. **Dean**. Thank you, Madam Chairman. It's a pleasure to be back.

Fusion Power Associates, as you know, is a nonprofit, tax-exempt corporation which was established in 1979 to insure the timely development of fusion as a practical energy source. We have 52 industries, utilities, and universities as members and affiliates. I would be pleased to submit the names of our membership and my written testimony for the record.

The Department of Energy's budget is primarily for the continuation of the strong efforts that they've had in tokamak and magnetic mirror confinement activities, combined with supporting research in other areas, such as basic research, technology, and alternate concept development.

The Department originally requested $505 million from the Office of Management and Budget for this program in fiscal 1984, and I would urge the Congress to restore that figure for the 1984 budget.

You might ask what would this $40 million, approximately, increase be spent for? I think many people have lists of how this money might be spent—$10 million here, $5 million there—and that is certainly one way to do it. And all of these ideas that I have heard are for worthy projects. I would suggest that you might consider another alternative, also. Dr. Clarke, in making recent speeches, such as at our symposium in San Diego, described the fusion program as being that of a "base program" and a "reactor engineering program." He said that the President's budget supports a base program but does not support a reactor engineering program. I think that is one of the major differences between this committee's views and the Fusion Act, and the Department of Energy and the administration; this difference between running a focused reactor engineering program to develop a practical product and running a base research and development program.

I would urge you to consider making an add-on of $40 million, or whatever number you can come up with, and asking the Department how it would spend it on a reactor engineering program, and only authorizing that money if the Department comes up with a reactor engineering program that really addresses itself to moving toward the purposes of the act for developing a commercial product, not allowing them to just spread it around on existing facilities and existing technology development programs but really packaging it in a way that is responsive to trying to develop a commercial fusion product expeditiously.

One of the major features of the President's submission to you is the calling out for cancellation of the line item for the EBT construction project. The delay in getting that project going has resulted in what often happens in R&D programs, which is that the science evolves and certain questions have been raised about the scaling laws in EBT which require some relook at the design of the EBT project and will further delay its construction, as is now appropriate. However, I would urge the Congress not to simply cancel this line item. If there has to be a delay while it is redesigned and while it is decided as to what its cost should be and when it can really go into fabrication and construction, that is all well and good. But I do not believe that it would be appropriate to simply cancel the line item which has been won at very great time and cost to many people, including the expenditure of millions of dollars by private industry to compete for this, the designation of a private location for it in the city of Oak Ridge, and the selection of a prime contractor.

I believe these are important achievements that are important for the fusion program as a whole. I believe this program eventually will and should go forward.

We do meet today at a time of great expectation in the fusion program. The TFTR has recently been completed, and we expect great things from TFTR during the rest of this decade. However, I think all of us are deeply concerned that there are no programs being laid in place today that will allow us to have improved capability in the 1990's. Therefore, one of the most important things I believe needs to be done is that we have to start programs that are seriously directed toward improved facilities that will operate in the 1990's.

In some cases this requires upgrades of present facilities, but in other cases it requires money for design and construction on an expeditious basis of new facilities which may or may not be located at present sites. These are the equivalent of what was called for in the fusion engineering device, but it may have another name or it may be something that is not quite yet designed. However, I believe we require—and this is not in the budget— adequate budgets and intent to design the kinds of facilities that we need in order to have improved capabilities in the 1990's.

I think that the U.S. Government's policy for fusion ought to be based on an intent to maintain U.S. leadership in a world effort. At the present time the United States represents about a third of the world effort, but of more concern to me is that in Japan and Europe the industrial skills required to engineer fusion facilities and to develop commercial fusion technologies—these are developing in industry much more rapidly in Europe and Japan today than they are in the United States. I believe that we must continue to try to convince the Department of Energy of the importance to develop these technical and engineering skills in our industry.

The science community and the engineering communities have reviewed fusion many times in the past several years, and in every case have certified to you that the program is ready to move into an engineering development phase. The Department's budget emphasizes and maintains a strong scientific program with considerable effort on general technology development, but does not propose to move the program significantly into engineering development with major responsibilities beginning to be shifted to industry. I believe that was an important principle of the Fusion Act and that the Congress should continue to urge the Department to implement the principles that are advocated in the Fusion Act.

I think the two most important things that the Congress needs to urge on the Department are the needs to seriously initiate activities aimed at building next-generation fusion power devices and also to nurture the development of an industrial base for fusion engineering and technology for the future.

Thank you.

[The prepared statement of Dr. Dean follows:]

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STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
MARCH 16, 1983

It is a pleasure to have the opportunity to address your subcommittee again this year as you consider the Department of Energy's FY 1984 funding request for fusion energy research.

Fusion Power Associates is a non-profit corporation established in 1979 "to ensure the timely development and acceptance of fusion as a socially, environmentally and economically attractive source of energy." Fifty-two industries, utilities, and universities are currently members or affiliates of Fusion Power Associates.

The Department of Energy request for magnetic fusion energy is primarily for continuations of existing strong efforts in tokamak and magnetic mirror confinement activities, combined with efforts in supporting research, technology and alternate concept development. The Department originally requested an FY 1984 fusion budget from the OMB of $505 million, a figure I urge the Congress to restore. Within the constraints of the $467 million budget request presented, I believe the scientific program presented by the Department is fairly well balanced and will provide continued scientific progress. As it has for the past two years, however, the Department gives undue emphasis to the scientific aspects, and insufficient emphasis to the engineering development needs, of the program. Faced with relatively constant funding, the Department has preferentially protected the scientific programs. As far as I can tell, the Department has no intention of shifting major responsibilities to industry, or to seek the expeditious development of a practical fusion energy source as mandated by Congress in Public Law 96-386, the Magnetic Fusion Energy Engineering Act of 1980.

The major initiative to bring industry into the mainstream of fusion development--the EBT-P Project--has been called out for cancellation. Several years ago, private industry was encouraged by DOE to spend millions of dollars of its own resources to compete for the EBT-P project. McDonnell Douglas Astronautics Company, the winner of the competition, purchased a site in the city of Oak Ridge, Tennessee, to construct the EBT-P facility. An imaginative new procedure for industrial participation in fusion development was laid in place, which it was hoped would set an example of how research spawned within the national laboratory system could lead to industrial development and eventual commercialization. The proposal to abandon this effort, if approved by the Congress, would be detrimental to building an industrial base for fusion development.

We meet today at a time of great expectation in the fusion program. The Tokamak Fusion Test Reactor (TFTR) was completed in late December by an industrial team headed by Ebasco Services, Inc. under contract to Princeton University. The availability of this test facility opens up major new opportunities for advancing both the science and the technology of fusion. Its authorization back in the FY 1976 budget cycle was an act of vision and faith on the part of the Congress and the Executive and should provide a lesson to us in our present circumstances. The TFTR should provide landmark new results throughout the rest of the decade. Unfortunately, there are scant signs today that we will have any new capability in the 1990's. This is not the fault of the Congress, which has fought valiantly these past two years to preserve momentum in fusion and in many other energy programs. We need serious design efforts leading to the initiation and construction of new and improved facilities which will outperform the TFTR and MFTF in the early 1990's.

Fusion progress continues on a broad front. A new Tritium Systems Test Assembly was recently brought into operation at Los Alamos and large superconducting coils are arriving from all over the world to be inserted into the new Large Coil Test Facility at Oak Ridge. Magnetic mirrors are making progress and have sparked increased interest, especially in Japan. Vigorous support to expedite the completion of the Mirror Fusion Test Facility (MFTF-B) is required, lest this important project experience needless schedule delays and resulting cost increases. Support is also required for the construction of the FMIT materials test facility in Hanford.

There is a growing interest in the so-called "compact fusion concepts", defined as "concepts which have higher power density and, for the same total power, are significantly smaller than a conventional tokamak or mirror reactor." Although the compact concepts potentially have many attractive features uncertainties in their performance and development paths are larger than in the case of the conventional concepts. Private sector interest is evident in the compact concept area.

In addition to supporting a continued strong effort in magnetic fusion, I urge your committee to press for a strong civilian component in the inertial fusion effort now funded by the DOE military program. Good progress is being made in inertial fusion. Funding requests for inertial fusion are inadequate to properly support the operations of existing facilities and to maintain the construction schedule of the Particle Beam Fusion facility, PBFA-II.

The U.S. Government should attempt to maintain U.S. technical leadership in the intense world fusion effort. At the present time, the U.S. program represents only about one-third of the world effort and fusion devices larger than TFTR are under construction in Europe and Japan. The industrial skills required to engineer fusion power systems are today developing more rapidly in Japan and Europe than in the U.S. The technological skills and accomplishments which result from pressing forward on the frontiers of science and technology have benefits in a wide range of near-term applications. Such "spinoffs" play a major role in the continued evolution of advanced technological society and depend on the existence of a dedicated, stable cadre of scientists, engineers and organizations. We must not ignore the need to nurture the evolution of fusion skills in our industry.

The scientific and engineering communities from both the public and private sector have certified to the government that the fusion program is ready to move into engineering development. The focused, practical goals provided by the Fusion Act are still desirable and achievable and in the best interests of the country. I urge the Congress to maintain the Fusion Act in its present form and to continue to urge the Executive to follow the principles layed down in the Act as expeditiously and faithfully as possible. Most urgent are the needs to initiate a next generation fusion power device and to nurture the development of an industrial base for fusion engineering and technology.

Mr. **Hall**. Thank you, Dr. Dean.

We will recognize Mr. McCormack.

STATEMENT OF MIKE McCORMACK, ENERGY CONSULTANT

Mr. **McCormack**. Thank you, Mr. Chairman. It is always a privilege and an honor for me to return to the Subcommittee on Energy Research and Production, and to share my thoughts with you.

My comments today focus on this Nation's magnetic fusion research, development and demonstration program; and amount to a philosophical discussion of the policies and programs we are following, and what they should be. In this respect, I wish to pay tribute to the members of this subcommittee with whom I served in the 96th Congress when we wrote and had enacted into law the Magnetic Fusion Energy Engineering Act of 1980. It was a truly significant accomplishment. In discussing the philosophy of the act, I hope that I may review for the new members of the subcommittee some of the congressional perspective concerning the magnetic fusion program when we enacted the law.

To paraphrase that perspective: there is really only one meaningful goal for this Nation's fusion program. That is the demonstration of competitive fusion power production at the earliest possible date. Every other activity or project within the fusion program is, at best, secondary to this one fundamental goal: competitive fusion power as soon as possible. It is our stated goal in the law, and none of us should lose sight of it. Fusion research is not an end in itself, and success in any specific fusion program has meaning only if it contributes—or is meant to contribute—to reaching that goal.

Most emphatically, we are not talking about some leisurely research program that may bring fusion power in 70 years.

It must be remembered that this Nation's survival as a viable political and economic entity with adequate standards of living for our people, and protection of the various environments of concern to us, depends upon a continually increasing electricity production capacity for as far as we can see into the future, even with extraordinarily successful spartan conservation programs. Until fusion power becomes available, at least 90 percent of the new energy produced in this country must come from coal and nuclear fission. Fusion electricity production must move into this arena as soon as possible. Only the reasonable chance that we will succeed in fusion power production within a foreseeable timeframe justifies present expenditure levels for magnetic fusion research, to say nothing of the expenditures that we have projected.

The Magnetic Fusion Energy Engineering Act of 1980 sets out certain intermediate goals which lead to the operation of a magnetic fusion electric demonstration plant by about the year 2000. My own perspective is that even this date—2000—is dangerously late as far as the critical energy needs of this country are concerned. The law states:

> Sec. 2. (a) The Congress hereby finds that—
>
> (6) to ensure the timely commercialization of magnetic fusion energy systems, the United States must demonstrate at an early date the engineering feasibility of magnetic fusion energy systems;

(9) acceleration of the current magnetic fusion program will require a doubling within seven years of the present funding level without consideration of inflation and a 25 per centum increase in funding each of fiscal years 1982 and 1983.

(b) It is therefore declared to be the policy of the United States and the purpose of this Act to accelerate the national effort in research, development, and demonstration activities related to magnetic fusion energy systems. Further, it is declared to be the policy of the United States and the purpose of this Act that the objectives of such program shall be—

(3) to achieve at the earliest practicable time, but not later than the year 1990, operation of a magnetic fusion engineering device based on the best available confinement concept;

(4) to establish as a national goal the operation of a magnetic fusion demonstration plant at the turn of the twenty-first century;

(5) to foster cooperation in magnetic fusion research and development among government, universities, industry, and national laboratories;

(6) to promote the broad participation of domestic industry in the national magnetic fusion program

* * * * * * *

Section 5. (a) The Secretary shall prepare a comprehensive program management plan for the conduct of the research, development, and demonstration activities under this Act.

(b) The Secretary shall transmit the comprehensive program management plan to the Committee on Science and Technology of the House of Representatives and the Committee on Energy and Natural Resources of the Senate not later than January 1, 1982.

* * * * * * *

Section 6. (a) The Secretary shall develop a plan for the creation of a national magnetic fusion engineering center for the purpose of accelerating fusion technology development via the concentration and coordination of major magnetic fusion engineering devices and associated activities at such a national center.

(c) The Secretary shall submit not later than July 1, 1981, a report to the House Committee on Science and Technology and the Senate Committee on Energy and Natural Resources characterizing the plan and setting forth the steps necessary for implementation of the plan, including any steps already implemented.

It should be emphasized that this legislation was not just an exercise in congressional courtesy—nor was it an election-year goody. Both Houses of the Congress supported—almost without dissent—the 20 yea, $20 billion program established in the law—recognizing that success will make an inexhaustible clean energy source available to all mankind. The law was based on the conclusions of three prestigious committees of the nation's outstanding scientists and engineers: the Foster Committee, the Hirsch Panel, and the Buchsbaum Panel.

The Report of the Foster Committee stated in part:

> In our judgment the proper strategy should optimize the chances of determining the highest potential for a commercial fusion energy source at the earliest practical date.
>
> * * * it is too risky at this time to concentrate just on the physics experiments and delay to a later time considerations of downstream engineering problems.
>
> * * * there must be a major increase in emphasis on engineering problems to provide the basis for proceeding to engineering development and the subsequent choice of an engineered prototype reactor.

Following the Foster Committee Report, a Department of Energy Policy Statement on fusion included the following:

> * * * there is no lead time to spare: if fusion energy is to be available when it is needed—the research and development program, rigorously directed toward the goal of commercial utility, must be undertaken in earnest now.
>
> * * * the goal of fusion research in the Department of Energy is to develop the highest potential for employment of fusion energy.

In 1980 the Hirsch Panel—advising the House Committee on Science and Technology—stated:

> The time is now for an engineering thrust centering on an engineering test facility.
>
> Our panel believes that fusion can be made commercial before 2000 if a national commitment is made soon.

In response to the Hirsch Panel report and the initiatives of the Committee on Science and Technology, the Department of Energy constituted a new panel, chaired by Dr. Sol Buchsbaum. After many man-days of intense study, the Buchsbaum Panel endorsed the concepts of the bill, and of a magnetic fusion electric demonstration plant by about the year 2000, and stated:

> The magnetic fusion program can, and should, embark on the next logical phase toward its goal of achieving economic feasibility of magnetic fusion. To this end a broad program of engineering experimentation and analysis should be undertaken under the aegis of a Center for Fusion Engineering (CFE).
>
> A new element of the program should be a device containing a burning plasma, and incorporating in its construction those technological features which can serve as a focus for the development of future reactor technology * * *

I should also observe the ultimate compliment to our Nation in enacting the Magnetic Fusion Energy Engineering Act of 1980. Within weeks, the Japanese Government and the European Economic Council undertook reviews of their respective fusion programs; and, within a few months, each review recommended an accelerated program for magnetic fusion, including emphasis on engineering development, materials testing—and the early construction of a fusion engineering test facility, just as we in the 96th Congress had concluded.

The law calls for the creation of a Center for Fusion Engineering which would integrate and direct the programs by which we would reach our goal of fusion power demonstration. Unfortunately the Center for Fusion Engineering has been sidetracked—as a matter of policy—by this administration. This is a major misfortune for our country. It will delay—unnecessarily—the success of the Federal fusion program, and make it much more difficult for us to achieve our goal.

The law also calls for an engineering development and materials testing program, based substantially on early completion of the fusion materials irradiation test facility. Both the program and the partially completed FMIT have been delayed. The longer we delay the FMIT, the longer we must wait for a successful next step in the fusion engineering development program.

Mr. Chairman I am deeply concerned with the funding level requested by the administration for magnetic fusion research, development, and demonstration for fiscal year 1984. The law called for a spending level of $525 million for fiscal year 1982, as compared to the $451 that was exactly spent. It called for a 25-percent increase above the $525 for fiscal year 1983. That would have amounted to $656 million, as compared to the $447 we are spending under the continuing resolution this fiscal year. Accordingly, the funding level for fiscal year 1984, if the program were being carried out as directed under the law, would be more than $800 million, as compared to the $467 million requested by the administration.

I should observe that the TFTR at Princeton and the MFTFB at Livermore are the last major machines scheduled for construction under the administration's program, and that funding levels for construction have dropped from $114 million in fiscal year 1982 to a requested $36 million for fiscal year 1984.

The funding for the FMIT is especially critical. This facility would cost about $120 million to complete. Representatives from the Japanese Government and the European Economic Community have indicated a willingness to help finance this project, but are obviously waiting for some sign from the United States that we ourselves are serious about completing the facility.

Mr. Chairman, the results of the administration's reduced budget for magnetic fusion are farther reaching than simply spending less. The nature and the philosophy of the fusion program is being changed back toward what Secretary Schlesinger called a scientific sandbox, in which plasma physics projects at the large laboratories are liberally funded, but the commitment to develop the engineering technology of fusion, and the projects to provide the facilities to develop that technology, are being ignored or dried up.

It is critically important for the members of this committee to recognize that not a single Member of Congress has been involved in this decision to ignore the law and abandon its goals.

Now, I'm not so unrealistic, Mr. Chairman, to suggest that this subcommittee attempt, in the present budgetary atmosphere, to authorize funding the magnetic fusion program at—or even near—the legally prescribed level.

However, I do have several realistic proposals for your serious consideration:

One, I suggest that the FMIT be funded at $10 million for fiscal year 1984, and that the Department of Energy be directed to request $5 million each for the FMIT in fiscal year 1984 from Japan and the European Economic Community, with a commitment from the United States that we will continue to expeditiously complete the project, if each of these two partners will contribute 25 percent each year for construction and operation of this facility. They will, of course, share in the information obtained at the FMIT.

Two, I suggest that strongly worded language be included in the House Science and Technology Committee report—and called to the attention of all Members of Congress—addressing the fact that the administration is not following the intent of the Magnetic Fusion Energy Engineering Act of 1980; and calling upon the administration to move forward promptly with a realistic program to implement this law, even if it cannot be funded at this time at the levels outlined in the law.

Three, I suggest that the authorizing legislation that you will report insist that the administration initiate now a program which involves private industry in planning and problem-solving with respect to reaching our goal of fusion power production. A thorough review should be conducted in the near future as to the most realistic steps to be conducted in the near future as to the most realistic steps to be taken cooperatively by the Department of Energy and the industrial community which will lead to the construction and operation of a fusion engineering test facility at the earliest practical date.

Four, I suggest that a small team of experts be assembled to report directly to Dr. Alvin Trivelpiece, Director of Energy Research, and that their job be to explore every avenue of cooperative effort between the Department of Energy and private industry in moving toward a program of engineering development and materials testing for magnetic fusion, and the development of a fusion engineering test facility.

In essence, without adding significantly to the budget, the subcommittee would be recommending the creation of a team, reporting to the Director of Energy Research, which would accomplish much of the mission originally proposed for the Center for Fusion Engineering.

In this respect, Mr. Chairman, we should observe that the large research machines such as the TFTR and the MFTFB may not lead us to a practical power producing fusion device. The question that we must ultimately face, with respect to these devices is not "will they work"—because they will—but rather "will they lead to large power producing machines."—I think they will not—although they are valuable as research devices.

It may be most appropriate now to consider the potential of small machines, such as GA technology's OTHE or INESCO's RIGGATRON, either or both of which may demonstrate economically competitive electricity generation by the early 1990's. Since this is a possibility, the Department of Energy should be directed to plan accordingly, and to take the appropriate steps to accommodate such a development, cooperating with small private companies that may succeed, always remembering our ultimate goal. I am encouraged by recent indications that the Department of Energy is interested in exploring this potential. I hope the Department will follow through. There is much generic work of great value to be done. Mr. Chairman, I should inform you at this point that I am a paid consultant for INESCO Corp.

Mr. Chairman, I am confident that the Congress will not be diverted from our primary goal, or persuaded by anyone that the Magnetic Fusion Energy Engineering Act of 1980 should be unilaterally dismissed. Your obligation, as members of this committee, should be clear: To insist that the law be observed—and that its goal of fusion power production at the earliest practical moment be kept inviolate.

Mr. Hall. Thank you, Mr. McCormack.

Dr. Mense, we will now recognize you for your testimony.

STATEMENT OF DR. ALLAN T. MENSE, SENIOR SCIENTIST, FUSION SCIENCE DEPARTMENT, McDONNELL DOUGLAS ASTRONAUTICS CO.

Dr. Mense. Thank you, Mr. Chairman.

I am Allan Mense. I am a senior scientist with the Fusion Science Department of the McDonnell Douglas Astronautics Co. and adjunct professor of physics at the University of Missouri.

Today, though, I am appearing before you to present testimony on behalf of both the energy and research and development committees of the Institute of Electrical and Electronics Engineers.

We are the world's largest engineering society, with over 220,000 members, 185,000 of whom live and work and pay taxes in the United States.

Although my testimony today will focus on fusion energy, the institute has policy positions on a number of areas that are within the oversight responsibilities of this subcommittee. With your permission and no objection, we would like to submit these policy position papers as appendices in my written testimony today.

Mr. **Hall**. They will be accepted without objection.

Dr. **Mense**. What I would like to do now, if I could, is to submit my written testimony for the record then and briefly summarize what is in the testimony in order to not take up too much time.

It is always a pleasure to be the last person on the panel because I can always use "ditto" and "I agree with," which makes most of the other panel members happy to know that I agree with what they have had to say.

Certainly the scientific status of the fusion program is something that we will all agree is at a state of scientific readiness. That is not to say that there are not some key scientific issues which still must be resolved.

One of the points which I think we have to be concerned with, besides the scientific readiness, is what I would call the technical readiness of the program. By that I mean the technical readiness of the program to move into an engineering phase in which we can actually determine whether or not useful power can come from fusion.

The additional technology programs which are needed are ones that I believe can be fostered and nurtured in a reasonable way with a budget which could grow at whatever level the Congress and the administration believes they can afford.

Right now there is only a small amount of meaningful industrial participation in the program, and that is in the EBT-P project. If budgets remain flat for the next 3 to 10 years, we would forecast that there would be no meaningful industrial participation in the fusion program, which means that at the time in which you may wish to move forward with the program the Nation would have to go through a tremendous learning curve again. I call to your attention the situation in Britain where the atomic energy research establishment has traditionally been in charge of building fission reactors. Then, under Mrs. Thatcher not too long ago she said, "By golly, we're going to have industry build us a fission reactor." There are cost overruns all over the place.

I think one of the things to realize is that there are no magic, superhuman beings in industry. We have to learn just like everyone else, but we need to be included early in the program and at every phase of research and technology development. We would hope that this committee would try to make sure that we are involved with that.

In your letter of invitation to testify, one of the points which we were asked to address is the necessity for maintaining the great bulk of the program's funding in the scientific phase rather than initiating significant engineering development activities and consolidating and reducing efforts.

Right now within the fiscal year 1984 budget level of $467 million, we believe there is little, if any, room to perform meaningful engineering development. It is probably wasteful to pursue engineering of fusion devices without maintaining a strong science data base, and the science data base is still evolving. It is a little like trying to build a higher pyramid by removing bricks from the base. The narrow base brings the risk of producing an unstable structure. So, too, could that happen with the fusion program if you try to collapse the science program strictly in favor of engineering.

You also asked for us to list specific program activities that are not contained in the 1984 budget. Many of them have been outlined by the other people on this panel. The EBT-P project was zeroed in the fiscal year 1984 budget. This is the only project line item which involves major industrial participation. The industrial participant in this project was prepared to provide $10 million of their own company money to provide the facility to house the project. This kind of investment, 20 years prior to any reasonable return on investment, is certainly courageous to say the the least. We would hope that this committee would look into the appropriateness of dropping this project in fiscal year 1984.

The other concern for us is the fusion materials radiation test facility which is at the Westinghouse Hanford Engineering Development Labratory. Our concern there is slightly different from that voiced by others here today. We are very much concerned with this country coopting its technology innovation to foreign governments for a few cents on the dollar. You could build—the United States could build, with U.S. money, this entire facility—this is a technology facility—for very little in the way of real dollar increases in the fusion budget.

A cooperative program in fusion science is very good, and that has been a strong base in the fusion program ever since the 1960's when it was declassified. That should be continued.

However, as we get closer and closer to technology development in which there become concerns for patent rights and proprietary things and ideas and spinoffs which can find their way into the marketplace through U.S. manufacturers, then we get very concerned when, for just small budget differences, we have to go out and try to seek foreign funds. We are letting them buy our technology for 10 cents on the dollar.

We would like the committee to really give some serious thought into whether or not we can or cannot afford that project. We believe that the country must afford such a project. It is very vital.

Another point in your letter was your concern about the desirability of amending Public Law 96-386 to reflect the lengthened schedule of fusion energy development. It is our institute's belief that any direction taken by Congress to reassert to the administration its desires and intentions will have a positive impact on the program.

What has been perceived by the administration is that Public Law 96-386 is only what the 96th Congress thought. What is the 98th Congress going to say? Our feeling is that, while in times of tight budget constraints, one may not find as objective a consensus from the fusion community as to what is exactly the right thing to do next, we believe that by applying the correct set of, what I will call, congressional filters, new or amended legislation would have a beneficial effect.

You asked about what is the appropriate level of industrial involvement in the fusion program. I think this is a complex issue and it is intimately tied up with whether or not fusion will be allowed to retrench and climb into its basic science mode every time the going gets tough. If you allow it to retrench into a basic science mode and continue to drop the engineering technology, which is what the industry is involved in, then you are going to continually be performing a disservice both to the country and the industry which serves it.

Therefore, we would tend to believe that the most important thing is to try to provide some steady funding.

One of the things we want to do—that the program should do—is, of course, cultivate industry, but this takes laboratory resources. The learning process for industry requires time, and, of course, there is a great reluctance on the part of the laboratories to undertake it, particularly under severe budget restraints. I think that is all well and good and needs to be understood. But we also need to understand that tight budgets complicate the technology transfer process by invoking concern over future laboratory jobs. It is not clear how this concern can be ameliorated.

The industrial participation program has sought industrial involvement but has not resulted in many meaningful partnerships to date. We would say the most utilitatian mechanism for increasing industrial involvement would be to move the program into an engineering development phase. But, in light of the economic constraints that we are under right now, probably the best one could do would be to try and restore the program funding level to one in which you have some forward momentum. That would require an increase in the fusion budget on the order of 10 percent.

It should be clearly remembered that fusion is still a long-term, high-risk venture. That is obviously why the Government should and does play a primary funding role. The high-risk nature of the program is not, however, associated with whether or not net energy-producing fusion reactors can be built. That is not where the high risk is. The high-risk factor comes about when one asks the question whether or not the first confinement device that achieves these required science parameters will be one which can be extrapolated to an economically attractive powerplant. That is where the high risk is.

I think a lot of us, when we saw Three-Mile Island, wished that maybe the country had done a little more work on high-temperature, gas-cooled reactors. I am sure at least one other person at this table would agree with me on that.

Dr. AGNEW. Two.

Dr. MENSE. At least two. As a matter of fact, I would probably say five.

Therefore, I think what one has to ask is, are there affordable ways to allow the engineering part of the program to go forward and yet maintain yourself with a reasonable backup in the way of ideas? I think the answer is yes. I think it basically requires about 15 percent of your yearly fusion budget to maintain a backup program that will provide strong science in concepts other than the mainline programs.

The United States has spent nearly $3 billion to date on fusion research and will probably need to spend $10 to $20 billion more before a demonstration plant is on line. Thus, fusion is a long-term investment. To drive this point home, let me tell you a little story about a young man who was eager to buy his first car.

He was interested in speed and style and color. He was anxious to buy a fast, sleek, red sportscar that was sitting on the dealer's showroom floor. There was, alas, one problem. The young man needed a loan to buy this car. Since his salary was fairly low, he needed a long-term loan to keep payments within his budget.

He found that all the banks and even the auto dealers charged too high an interest rate, and his father, quite frankly, did not have the cash. The young man sought the counsel of his father, who recommended that he talk with his sometimes not very pleasant but always concerned uncle. Let us suppose his uncle's name was "Sam."

Well, Uncle Sam was not impressed with all the criteria the young man used to decide on how to buy a car. He had not looked at reliability, the quality of the workmanship, the length of the warranty. He had not talked to other owners of automobiles to see what were important considerations in carownership. Most importantly, his uncle asked him about how long the car would last in the severe weather which was always present in that part of the country. The young man did not understand his uncle's concern.

The uncle patiently replied, in a manner making the message very clear, "Nephew," he said, "if you are going to take out a long-term loan, you don't buy a short-term car."

I believe the parallels to fusion and the various reactor confinement concepts need little elaboration. As yet, we do not know which of the concepts will turn out to be short term and which will be long term. For modest and reasonable funding patterns over the next 5 to 10 years, we believe that you can maintain strong alternate concepts program and still move the engineering part of the program forward, so we can know, as soon as possible, whether or not we can, in fact, derive energy from the fusion process.

I have included in my testimony a number of other concerns of the institute. Let me just highlight one or two.

We are concerned with the fact that in Japan 80 percent of the government funded fusion research is in industry. We are concerned that Japan's fusion budget has grown by 38 percent per year since 1977, and the United States budget has grown by only 6 percent per year.

We are concerned with the fact that the joint European tokamak, which will come on line sometime this year, just a matter of 6 to 10 months behind our device, is tremendously more aggressive than our tokamak fusion test reactor recently completed at the Princeton Plagman physics laboratory.

One of the important points here is the European Economic Community has budgeting cycles that allow their people to know roughly what their budget is for 5 years ahead. When it comes to doing major projects, that is a very, very important thing to have. We would hope that the Congress would try to come up with some creative authorization and appropriating mechanisms which will allow long-term programs, such as fusion, to proceed in an orderly way without cost overruns.

We are also concerned with the fact that energy R&D in general is taking a back seat and is being written off as a problem which the marketplace will solve. To believe that the marketplace has the ability to understand long-term R&D needs is unrealistic. The marketplace does not set national directions, nor plan national goals or objectives. The marketplace responds to those forces, most of which have historically come from Government policies. The marketplace cannot, and therefore does not, lead the world. It follows it—and provides a reasonably fair mechanism for achieving some national and international objectives.

The direction of technological innovation as well as economic behavior, and even social reform, depend critically on Government awareness and attitude. This can be seen especially in energy R&D.

A continued effort to provide this country with the energy security it so desperately needs must be the long-term goal targeted by this committee's actions today. The institute applauds your history of impressive action and encourages your continued leadership.

Thank you very much, Mr. Chairman.

Mr. HALL. Dr. Mense, thank you.

[The prepared statement of Dr. Mense follows:]

TESTIMONY
from
Dr. ALLAN T. MENSE
before the
ENERGY RESEARCH AND PRODUCTION SUBCOMMITTEE
of the
SCIENCE AND TECHNOLOGY COMMITTEE
U.S. HOUSE OF REPRESENTATIVES
16 MARCH 1983

I am Allan T. Mense, Senior Scientist with the Fusion Science Department of the McDonnell Douglas Astronautics Company. Today I am appearing before you to present testimony on behalf of the Energy and Research and Development Committees of the Institute of Electrical and Electronics Engineers (IEEE). The IEEE is the world's largest engineering society, with 220,000 members worldwide, 185,000 of whom live and work in the United States. Although my testimony today will focus on fusion energy research, the Institute itself has policy positions on a number of other areas which are within the oversight responsibilities of the Subcommittee. With your permission, policy position papers in these other areas will be included as appendices to my written testimony.

In your letter of invitation to testify, you outlined five areas which you would like specifically addressed. Our comments on these issues are provided below.

ISSUE 1 Assess the scientific status of the fusion program and its technical readiness to move into a significant engineering development phase.

The scientific status of fusion remains reasonably solid. While each of the confinement concepts still has the need to resolve some key science problems, there appears to be no insurmountable physics issues that would inflict an unacceptable risk into the program's ability to proceed with the design and construction of the next generation of devices. The technical readiness of the program is also sound, but can be improved through selected enhancement of key technology programs (e.g., radio frequency heating, superconducting magnets, materials). Unfortunately, the program may soon loose the years of technical understanding accumulated by Industry on the Tokamak Fusion Test Reactor construction project at Princeton. There are only a few industries at present in the magnetic fusion business who have been able to retain, in active fusion work, the nucleus of engineering and management talent necessary to assist in the design and development of the next generation fusion devices. There will be little meaningful industry involvement other than as a parts supplier if the fusion budgets remain level for the next 3 to 10 years. Thus, the magnetic fusion program is slipping its technical readiness and, due to policy pressures, the program is being forced to retreat back into a cocoon of basic science research. The difference between a program in retreat and one which can sustain reasonable forward momentum is about a 10% difference in budget. This will be addressed in more detail later in my testimony.

ISSUE 2 What is the necessity of maintaining the great bulk of the program's funding in the scientific phase, rather than initiating significant engineering development activities and consolidating and reducing efforts in the scientific phase of the program.

For the budget levels which the administration is requesting (FY 1984 - $467 million or $2 million below the FY 1983 appropriation), there is little, if any, room to perform meaningful engineering development. It is probably wasteful to pursue engineering of fusion devices without maintaining a strong science data base. The science data base is still evolving. It is a little like trying to build a higher pyramid by removing bricks from its base. A narrow base brings the risk of producing an unstable structure. We would not recommend trying to redirect funding from science to engineering. What we would recommend is that in addition to current funding levels to sustain a healthy basic fusion science effort, resources be allocated to begin moving the technology into the engineering development phase. Simply put, the program could very productively use a higher funding level. It is important to remember that since 1977 the magnetic fusion program budget has decreased by nearly 30%, if we measure it in 1977 dollars. This is even more of a loss in buying power for fusion due to its need for state-of-the-art equipment whose costs increase faster than the consumer price index.

ISSUE 3 What specific program activities are not contained in the FY 1984 fusion budget request and what funding levels would be necessary to maintain an aggressive pace as envisioned in P.L. 96-386?

The Elmo Bumpy Torus - Proof of Principle (EBT-P) experiment, which is the only construction project involving industry in a major design/management/scientific role has been deleted. It is our understanding that EBT-P was included in the Department's original request to OMB. The Fusion Materials Irradiation Test (FMIT) Facility, to be managed by the Westinghouse-Hanford Engineering Development Lab, and which can play a very meaningful role in validating materials properties, has only marginal funding in FY 1984. There is a danger that it might be dropped from the program if supplemental funds from foreign governments cannot be found to help. This brings us to a point of grave concern. When budgetary forces cause an advanced technology program, such as fusion, to sell-off its projects (e.g. FMIT) to foreign countries for a few cents on the dollar, it is time to rethink the country's policies. Sharing science is a good thing, and open communications on fusion science has been a strong tradition in the fusion community since the early 1960's. But sharing our technological innovations at a fraction of the cost certainly must cause us pause.

In addition to the two projects mentioned above, the technology development side of the fusion budget has been prevented from initiating vitally needed new test programs. Radio frequency heating is an outstanding example, as is evidenced by the Europeans and Japanese planning for 10 to 25 Megawatts of radio frequency injection into their large machines in the 1985-86 time frame, a good 2 years ahead of the U.S. This lack of funding for technology initiatives demonstratesthe momentum loss which the program has suffered.

In terms of addressing required funding levels to enhance the program pace, we believe the Department of Energy could better furnish this information. The hearings associated with PL 96-386 did, however, outline a funding profile to accomplish major construction. This type of plan, if implemented now, would require a 25% increase in the FY 1984 budget with a gradual but continual rise in budget until the annual level was roughly double the present $467 million. This would take 5 to 7 years if done in a reasonable but aggressive way. There appears to be little hope that the present Administration will see its way clear to ask for this high a level of funding. As mentioned earlier, however, a 10% increase in the FY 1984 budget over the Adminstration's request would have the effect of changing the momentum of the program into a forward moving and, we believe, necessary direction.

ISSUE 4 What is the desirability of amending PL 96-386 to reflect the lengthened schedule for fusion energy development proposed by the Administration?

It is our belief that any direction taken by Congress to reassert to the Administration its desires and intentions will have a positive impact on the program. In these tight budget times one may not find as objective a consensus from the fusion community as to what is exactly the right thing to do next, but by applying the correct set of Congressional filters, we believe that new or amended legislation would have a beneficial effect. It should be noted, however, that the cost to the country will increase dramatically as the development schedule is lengthened, both in terms of direct dollars as well as the costs incurred through the loss of our competitiveness with foreign developers of fusion technology.

ISSUE 5 What is an appropriate level of industrial involvement in the fusion program and how should that involvement change with time?

The answer to this is somewhat complex because it is intimately tied up with whether or not fusion will be allowed to retrench and climb into its basic science clothes whenever the going gets tough. Since, for historical reasons, basic fusion science has been pursued by the National Laboratories (GA Technologies program being the exception), industry is a comparative newcomer. As a newcomer, there has not been a high confidence level built up to date. However, as long as this country believes that industry should manufacture and build the facilities for the public sector, then industry must be allowed to be acquainted with the science and the technology necessary to do so. If we are to reduce the possibility of cost overruns in major fusion projects in the future, industry must work in every phase of the fusion program. Much of this early industry involvement must be as a learner, under the guidance of the labs, but gradually, like a proud but reluctant parent, the labs must pass the project management and technology development control to industry.

Since the process of cultivating industry takes laboratory resources, and the learning process requires time, there is great reluctance on the laboratories' part to undertake it, particularly under severe budget restraints. Tight budgets complicate the technology transfer process by also invoking concern over the future of laboratory jobs. Though the Industrial Participation Program has sought industry involvement, it has not resulted in a meaningful partnership which will nurture an industrial capability for product design, development and fabrication.

The most utilitarian mechanism for increasing industrial involvement would be to move the program into a major engineering development phase. This would require some major redirecting of policy within the Administration, but our Institute believes this must be done if fusion is to be an energy option and if the U.S. is to derive the many value-added benefits which go with developing this high technology product.

In addition to addressing the points of your letter, we have a list of further concerns that we would like to share with the Congress.

- There is concern that the program optimize its balance. Balance between mainline and alternate concepts. Balance between labs/universities/and industry.

It should clearly be remembered that fusion is still a long-term, high-risk venture. This is obviously why the government should and does play the primary funding role. The high-risk nature of the program is not, however, associated with whether or not a net energy producing fusion reactor can be built. The high-risk factor comes about when one asks the question whether or not the first confinement device that achieves the required science parameters will be one which can be extrapolated to an economically attractive power plant. Since each and every one of the present approaches has its own potential fatal flaws, and since no one is completely confident as to which devices will be able to overcome these flaws in a reasonable way, it would seem prudent to continue active research at a reasonable budget level in the alternate concepts (e.g., Bumpy Torus, Reverse Field Pinch, Stellarator).

The U.S. has spent nearly $3 billion to date on fusion research and will probably need to spend $10 billion to $20 billion more before a demonstration commerical power plant is on the line. Thus, fusion is a long term investment. To drive home the point a little more, let me share with you the story of the young man eager to buy his first car. He was intcrested in speed, style, and color. He was anxious to buy a fast, sleek, red sports car that was sitting on the dealer's showroom floor. There was alas one problem, the young man needed a loan to buy this car, and since his salary was fairly low, he needed a long term loan to keep the payments within his budget. He found that all.the banks and even the auto dealer charged too high an interest rate and his father quite frankly didn't have the cash. The young man sought the counsel of his father, who recommended that he talk with his sometimes not very pleasant but always concerned Uncle. Let us suppose his Uncle's name was Sam. Well, Uncle Sam was not impressed with all the criteria the young man used to decide which car to buy. He had not looked at reliability, the quality of the workmanship, the length of the warrantee. He hadn't talked to other owners of automobiles to see what were important considerations in car ownership. Most importantly, his Uncle asked him about how long the car would last in the severe weather which was always present in that part of the country. The young man did not understand his Uncle's concern. The Uncle patiently replied in a manner making the message clear. "Nephew," he said, "If you're going to take out a long term loan, you don't buy a short term car."

I believe the parallels to fusion and the various reactor confinement concepts needs little elaboration. We as yet do not know which of the concepts will turn out to be short term and which are long term. For modest and reasonable funding patterns over the next 5 years or so, one can maintain the alternate concepts in a state of readiness and undoubtedly benefit the mainline concepts at the same time.

The IEEE would encourage the Congress, and this Subcommittee in particular, to remain steadfast in their determination to maintain not only a forward moving program momentum but continue a balanced effective, long term, University/Laboratory/Industry effort.

Additional concerns center on:

- o Present Administration Policy guidelines are that unnecessarily pushing back the timeframe in which fusion can realistically be relied upon as a source for commercial power.
- o The Administration's clear lack of appreciation as to how long a lead time is needed for technology transfer from the labs and universities to industry, and the importance of early involvement.
- o The fact that a Comprehensive Program Management Plan, as required by PL 96-386, is yet to be forthcoming. It is well over 1 year late.
- o The pace of U.S. fusion R&D compared to foreign programs, to wit:
  - * Japan - 80% of fusion research is in industry (government funded). Budget growth since 1977 has been 38%/year, although the level was held flat from FY'82 to FY'83.
  - * Europe - Joint European Tokamak (JET) is more aggressive than TFTR in the U.S. EEC budgets are funded 5 years ahead, which stabilizes project construction.
  - * U.S.S.R. Broadest based and longest effort in terms of manpower (4,000 U.S.S.R. fusion scientists, 2,500 U.S.A.).
- o The fact that the U.S. program has 90% of R&D in labs and has experienced less than 6%/year growth since 1977 and is being held flat between FY'83 and FY'84.
- o The fact that Energy R&D, in general, is taking a back seat and is being written off as a problem which the market place will solve.

Our comments on the market place are as follows:

To believe the market place has the ability to understand long term R&D needs is unrealistic. The market place does not set national directions nor plan national goals or objectives. The market place responds to those forces, most of which have historically come from government policies. The market place cannot and, therefore, does not lead the world - it follows it, and provides a reasonably fair mechanism for achieving some national and international objectives. The direction of technological innovation as well as economic behavior (e.g., stock market), and even social reform, depends critically on governmental awareness and attitude. This can be seen especially in Energy R&D. The political vision which this Committee has shown over the years has lead to the accomplishments of today. A continued effort to provide this country with the energy security it needs must be the long term goal targeted by this Subcommittee's actions today. The IEEE applauds your history of impressive action and encourages your continued leadership.

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IEEE POSITION PAPER

Subject:

Energy: Fusion Power

"An IEEE Position Paper is a document, issued in the name of the Institute, developed to express a formal opinion, by an IEEE Entity on a specific topic."— From IEEE Policy 14 (1979)

IEEE Entity Initiating the Paper: Energy Committee

Need for Issuing:

Intended Use:

Initial Distribution:

The IEEE endorses fusion power as a source of abundant fuel with reduced radioactive hazards and environmental impact, as well as the potential for improved operating safety. The IEEE urges the continuance of broad areas of international cooperation in promoting fusion power, and fears that the present funding levels, although significant, correspond to research-level programs, without adequate provision for the engineering development of demonstration devices leading to construction of full-scale, on-line power plants.

The Institute therefore urges substantial increases in funding levels for fusion power.

Subject:
Energy: Fusion Power
IEEE POSITION PAPER

Nature of Release: DRAFT ☐ PRELIMINARY ☐ *FINAL ☒

Approval By the IEEE Entity:

	Thomas E. Lee	Chairman	6/17/77
Signature	Name	Title	Date

Approval By the Corresponding IEEE Major Board:

	Franklin H. Blecker	VP TAB	DATE
Signature	Name	Title	Date

*Approval By the IEEE Executive Committee:

	Robert B. Saunders	President	9/17/77
Signature	Name	Title	Date

IEEE STATEMENT ON FUSION POWER

Fusion power is regarded by the IEEE as a highly desirable goal because of its potential advantages; abundant fuel, reduced radioactive hazards, improved operating safety, and reduced enviromential impact.

The eventual goal of pure deuterium fusion will provide mankind with a permanent inexhaustible fuel supply. The immediate goal of deterium-tritium fusion, utilizing lithium as a primary fuel, will provide an adaquate fuel supply for energy abundance for many centuries.

The currently considered most probable form of a fusion reator is the toroidal or closed magnetic configuration, the Tokamak being the member of this class most widly studied. Backups to this approach are the open magnetic configurations usually called magnetic mirrors and the laser-ignited or inertially confined microexplosive systems also under active investigation.

Although all methods have their enthusiastic supporters, and many objective observers believe some form of fusion power will succeed, it must be admitted that disappointing performance of future devices might prove them not economically feasible. Examples of possible problems would be defeat of intertial confinement by an adaquate duty cycle, defeat of mirrors by excessive internal circulating power, and defeat of toroids by insufficient operating pressure.

On the other hand, practical applications of fusion systems might be accelerated by hybridizing fusion with fission. Either nuclear system by itself suffers neutron scarcity. A combination produces neutronic abundance and allows systems of striking properties. Early applications of fussion-driven fissioning systems might provide an alternate source of fissionable fuel for light water reactors, and other hybrid systems offer an important alternative to the fast breeder for exploiting our vast depleted-uranium (tailings from enrichment operations) reserves.

The IEEE is aware of the broad areas of international cooperation, and urges that these programs be encouraged and supported because of the key role of fusion power in the long-term solution of world-wide energy problems. Addressing the situation in the United States, the IEEE fears that the present funding level, although significant, corresponds to a research-level program without adaquate provision for the engineering development of demonstration devices leading to construction of full-scale on-line power plants. To reach these goals in timely fashion would require substantial increases in support levels.

IEEE Energy Committee
Approval - June 17, 1977
Thomas E. Lee, Chairman, IEEE Energy Committee

IEEE Board of Directors
Approval - September 17, 1977
Robert B. Saunder, President, IEEE

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IEEE POSITION PAPER

Subject:

Energy: Breeder Reactors in the United States

"An IEEE Position Paper is a document, issued in the name of the Institute, developed to express a formal opinion, by an IEEE Entity on a specific topic."—From IEEE Policy 14 (1979)

IEEE Entity Initiating the Paper: Energy Committee

Need for Issuing:

Intended Use:

Initial Distribution:

The IEEE Energy Committee recommends that an aggressive program on breeder reactor technology be maintained. The IEEE Energy Committee further recommends that the Clinch River Breeder Reactor (CRBR) plan project be continued in order to acquire engineering as well as manufacturing experience and plant component performance data requisite to developing a US liquid metal fast breeder reactor (LMFBR) technology.

This recommendation does not endorse a commitment to full-scale commercial development of the LMFBR fuel cycle prior to resolving the issues of reprocessing, waste management, safety, and safeguards; nor does it oppose the exploration of alternative fuel cycles or strategies which may offer certain advantages over the LMFBR.

Subject: Energy: Breeder Reactors in the United States

IEEE POSITION PAPER

Nature of Release: DRAFT ☐ PRELIMINARY ☐ *FINAL ☒

Approval By the IEEE Entity:

Signature	Name	Title	Date
	Hilton U. Brown, III	Chairman, Energy Committee	12-11-79

Approval By the Corresponding IEEE Major Board:

Signature	Name	Title	Date

*Approval By the IEEE Executive Committee:

Signature	Name	Title	Date
	Leo Young	President, IEEE	5-10-80

IEEE ENERGY COMMITTEE
POSITION ON
BREEDER REACTORS IN THE UNITLD STATES

The IEEE Energy Committee recommends that an aggressive program on breeder reactor technology be maintained. The IEEE Energy Committee further recommends that the Clinch River Breeder Reactor (CRBR) plant project be continued in order to acquire engineering as well as manufacturing experience and plant component performance data requisite to devloping a US liquid metal fast breeder reactor (LMFBR) technology.

Contingent upon attaining satisfactory component performance, the plant project should be carried to completion and operated as a precommercial demonstration of an independent and dependable US breeder-based electrical energy supply alternative, utilizing the large, already mined supply of uranium -238.

The Institute's Energy Committee does not endorse by this recommendation a commitment to commercialize the LMFBR fuel cycle prior to resolving the issues of reprocessing, waste management, safety, and safeguards; nor does it oppose the exploration of alternative fuel cycles or strategies which may offer certain advantages over the LMFBR.

COMMENT

The earliest dates by which a first commercial breeder will be needed are variously estimated in the period from the late 1990's to the 2040's. Currently, only the LMFBR is sufficiently advanced technologically to become available for commercial deployment early in this period. Failure to demonstrate the technology can be dependably applied commercially, if required, would subject the US economy and security to unnecessary risks. The costs of minimizing these risks are relatively small. A completed demonstration of the technology with the CRBR will provide the minimum assurance that the US has at least one independent electrical energy supply option, i.e., the use of the domestic stocks of fertile uranium -238 remaining from prior US uranium enrichment. Additional breeder concepts should also be developed for possible demonstration and commercial deployment later in this period.

Continuation of the CRBR plant project will help to reassert the intent of the US to be an international leader in the development of peaceful nuclear energy and thus to preserve us influence in world councils concerning the establishment of adequate nuclear safeguards and the minimization of proliferation risks.

IEEE ENERGY COMMITTEE
APPROVAL - 12-11-79
Hilton U. Brown, III, Chairman Energy Committee

IEEE EXECUTIVE COMMITTEE
APPROVAL - 5-10-80
Leo Young, President IEEE

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IEEE POSITION PAPER

Subject:

Energy: Electricity in the United States

"An IEEE Position Paper is a document, issued in the name of the Institute, developed to express a formal opinion, by an IEEE Entity on a specific topic."—From IEEE Policy 14 (1979)

IEEE Entity Initiating the Paper: Energy Committee

Need for Issuing:

Intended Use:

Initial Distribution:

The position statement addresses the need for a national energy policy to be developed by the government, in cooperation with industry and technical and professional organizations. The availability of energy is noted to be fundatmental to the welfare and economic health of society.

The position recommends prompt action to promote utilization of coal and uranium for electric power generation, as well as major emphasis on conservation, efficiency, and reliability. Adequate public safeguards and environmental protection is urged, as well as the development of most favorable new and advance energy technologies.

IEEE POSITION PAPER

Subject:

Electricity in the United States

Nature of Release: DRAFT ☐ PRELIMINARY ☐ *FINAL ☒

Approval By the IEEE Entity:

Signature	Name	Title	Date
	Thomas E. Lee	Chairman, Energy Committee	4-5-77

Approval By the Corresponding IEEE Major Board:

Signature	Name	Title	Date
	Franklin H. Blecker	Vice President, TAB	

*Approval By the IEEE Executive Committee:

Signature	Name	Title	Date
	Robert B. Saunders	President, IEEE	4-17-77

Key aspects of this policy should include:

Coal and Uranium

- Recognition that conversion of primary fuels to electricity offers an excellent method of making the shift in fuel resources to coal and uranium with the least disruption to the economy, and with maximum flexibility to accommodate change.
- A commitment to uranium and coal as the only rational near and medium-term alternatives to our present heavy emphasis on oil and gas.
- A program to de-emphasize the use of oil and gas a primary energy sources. These non-renewable and limited resources must be preserved for transportation in the medium-term and especially, in the long-term for non-energy needs such a feedstocks for chemicals, plastics and fertilizers.
- A continued national effort through well planned, managed, and coordinated joint government/industry efforts to complete the development of breeder reactors to commercial reality. Breeder reactors can extend our uranium resources for hundreds and perhaps thousands of years.

Conservation, Efficiency and Reliability

- A program which emphasizes energy conservation taking due account of the environmental, economic, and social impact of changing patterns of consumption.
- Continued and accelerated programs to achieve higher efficiency and better overall economics through improved utilization of electrical energy in manufacturing, transportation and in commerical and consumer usage.
- Conversion, where appropriate, from oil and gas-fired processes to electrical systems for more efficient energy end use and to shift dependence from scarce fuels to relatively more abundant coal and uranium.
- An increased effort supported by accelerated R&D programs to improve reliability of electric power system equipment. This effort should consider the design, construction and operational aspects of electric power systems and recognize the trade off between efficiency and reliability.

Public Safeguards and Environmental Protection

- Continued and accelerated programs to minimize the possible adverse environmental impact of coal and nuclear-based electric energy generation systems.
- An increased research program directed at the satisfactory disposal of waste products from both the nuclear and coal cycles.
- An aggressive national and international effort to addresss the problem arising from the production of plutonium and other fission products in the nuclear fuel cycle and to find an acceptable technical solution for the safe handling of large quantities of fission products, and political and social solutions to deal with the threat of these materials to our peace and tranquility.

IEEE Position on Electricity in the United States Energy Economy

This statement is addressed to the perceived need for a national energy policy to be developed by the government in cooperation with industry and technical and professional organizations which will recognize the importance of electrical energy based on domestically plentiful coal and uranium sources in the short and medium term and advanced technology and virtually inexhaustible or renewable sources in the long term.

The availability of energy is fundamental to the welfare and economic health of society. The domestic oil and gas resources upon which the United States has principally relied for several decades are being rapidly depleted. The critical nature of the United States dependence on foreign sources of energy was made apparent by the 1973-1974 oil embargo. The research and development effort required to bring alternate energy sources to commercial scale production, the large capital investments required for energy systems, and the pervasive societal impact of energy development requires the formulation and implementation of a sound national energy policy if our country is to maintain its standard of living.

The United States presently relies on oil and natural gas to supply the major portion of its energy requirements. At present rates of consumption, domestic resources of these two fuels will be near depletion for all practical purposes by the end of this century. Consequently, if there is not a significant change in our energy systems, dependence on imports will continue to increase with concomitant economic and national security risks and difficulties. Similarly, shortages will develop more rapidly for critical and unique nonenergy uses of petroleum and natural gas. This is an unacceptable direction for the future.

Two primary energy sources, coal and uranium, are available domestically in sufficient abundance to supply a major portion of our energy requirements for a long time into the future. Coal supplies can be expected to last well into the 22nd century at the present and projected rate of consumption. Uranium for light water reactors can be expected to meet demand as a major source of energy well into the 21st century; and a much longer time for breeder reactors.

The IEEE recommends the prompt adoption of a national energy policy. Four basic elements of this policy are:

o Increased utilization of coal and uranium in electric power generation.

o Major emphasis on conservation, efficiency, and reliability.

o Adequate public safeguards and environmental protection.

o Development of most favorable new and advanced energy technologies.

Advanced Electrical Energy Technologies

- An increased emphasis on research, design, and construction of advanced transmission and distribution systems.

- Continued and accelerated programs, such as Magnetohydrodynamics (MHD), fuel cells, advanced power cycles, etc., to achieve higher efficiencies and better overall economics in the production of electrical energy from fossil fuels.

- Development of renewable site specific sources such as solar, wind, geothermal and other alternative energy sources on a scale commensurate with the realistic appraisal of their potential as well as environmental and economic aspects.

- A continued national effort to develop fusion as a potential principal future energy source for the long-range.

It is the belief of the IEEE Energy Committee that the following essential benefits will be derived from the adoption of a national energy policy based on the above considerations.

- Reduced dependency upon foreign sources of energy resulting in:
    a. Reduced exposure to international political instability.
    b. Reduced pressure on the US balance of payments, and hence, reduced inflationary pressure.

- Assurance that adequate sources of energy will be available to meet the requirements for sustained economic health and an improved standard of living for that significant portion of our population which is now living at substandard levels.

- Significantly reduced adverse environmental impact through the substitution of electrical energy for fossil fuel combustion in end use consumption.

IEEE Energy Committee
Approval - 4-5-77
Thomas E. Lee

IEEE Board of Directors
Approval - 4-17-77
Robert B. Saunders

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IEEE POSITION PAPER

Subject:

Energy: The Need for Nuclear Power

"An IEEE Position Paper is a document, issued in the name of the Institute, developed to express a formal opinion, by an IEEE Entity on a specific topic."—From IEEE Policy 14 (1979)

IEEE Entity Initiating the Paper: Energy Committee

Need for Issuing:

Intended Use:

Initial Distribution:

The Board of Directors of the Institute of Electrical and Electronics Engineers, Inc. hereby goes on record as supporting the rapid and orderly development of nuclear electric power.

The supply of energy to meet worldwide needs can be achieved only by making use to the fullest extent of all the energy options, including wise conservation.

The limited size of known oil and gas reserves, relative to the world's energy requirements, establishes a pressing need to reduce dependence on these sources for the generation of electric power. While there is a vast potential in options such as solar energy and nuclear fusion an appreciable period of time is needed in their development. Therefore, it is deemed necessary to increase emphasis for the remainder of this century on the use of coal and fissionable fuels to provide the bulk of our energy needs. While additional engineering and environmential problems exist with the use of coal and fissionable fuels, these are receiving active attention and evolutionary refinements continually take place. Experience to date indicates that coal and uranium provide unique and practical alternatives to oil and gas. In accord with the need expressed above to develop all energy options, the utilization of fissionable fuels should proceed along with the increased use of coal.

Subject:
Energy: The Need for Nuclear Power
IEEE POSITION PAPER

Nature of Release: DRAFT ☐ PRELIMINARY ☐ *FINAL ☒

Approval By the IEEE Entity:

Signature	Name	Title	Date

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Signature	Name	Title	Date

***Approval By the IEEE Executive Committee:**

	Joseph K. Dillard	President	1-29-76
Signature	Name	Title	Date

The development of safe, efficient nuclear power plants has progressed rapidly and such facilities are now providing growing percentage of the world's electric power with an outstanding saftey record. This development is proceeding in an orderly, highly-requlated manner to insure that the public safety, the disposal of waste material, and the security of fuel are taken into account properly and adequately. Therefore, any energy policy should include the increased use of nuclear energy for elecric power generation.

The Board of Directors of The Institute of Electrical Electronics Engineers, Inc. urges all interested and responsible individuals and organizations to take actions to ensure that in addition to developing other energy sources, the rapid and orderly development of nuclear power be stimulated.

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IEEE POSITION PAPER

Subject:

Nuclear Waste Management

"An IEEE Position Paper is a document, issued in the name of the Institute, developed to express a formal opinion, by an IEEE Entity on a specific topic."— From IEEE Policy 14 (1979)

IEEE Entity Initiating the Paper: The Energy Committee

Need for Issuing: Anticipated Legislation Addressing the Issue

Intended Use: Legislative and Regulatory Hearing Records

Initial Distribution: Cognizant Committees in the Senate & House

Subject:

Nuclear Waste Management

IEEE POSITION PAPER

The Federal government must reaffirm a positive commitment to nuclear energy and support this commitment by the establishment and implementation of a well-defined waste management program. The federal government should assume broad oversight responsibility for all aspects of nuclear waste management through a single federal agency. The commercial nuclear power industry should retain responsibility for spent fuel and low-level wastes temporarily stored on site and at other locations on its properties. The disposal of low-level wastes generated by the nuclear power industry and other civilian activities should remain under the control of the states, subject to federal regulatory standards and guidelines and specific federal authorization of interstate (regional) compacts. The policy on spent nuclear reprocessing and recycling should be separated from the proliferation issue and resolved on the basis of economic costs, benefits, and environmental impact. Acceptance of spent fuels from foreign countries for interim storage should be minimized until a decision is made to resume reprocessing and adequate away-from-reactor (AFR) storage and disposal facilities are available. Civilian and defense waste issues should be integrated into a single system subject to the limitations of legitimate security consideration. A national plan for nuclear waste management must include effective state and public consultation processes.

Nature of Release: DRAFT ☐ PRELIMINARY ☐ *FINAL ☒

Approval By the IEEE Entity:

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IEEE ENERGY COMMITTEE POSITION ON NUCLEAR WASTE MANGEMENT

Nuclear energy is a major element of our national energy policy. It is vital to the public welfare and national security. Resolution of the nuclear waste issue is necessary to the continuation and growth of this energy source. A national policy and commitment to achieve a technically, politically and socially acceptable solution is urgently needed.

The technology capability is adaquate as a base for national policy. It may require extensions of existing knowledge but no scientific breakthrough. There is no evidence that remaining uncertainties are of sufficient concern to warrent relinquishing further commitment to nuclear energy. A continuing search for "best" solutions to specific problems, such as site selection, is non-productive. Further development should be mission-oriented with major emphasis on large-scale engineering demonstrations

Institutional and political issues are as important and more intractable than technical problems. They must be resolved to achieve an effective national program. The continuing failure of the federal government to pass needed legislation and provide direction on the disposal of high-level wastes and spent reactor fuel and to reconcile national needs with state and local interests concerning transportation and repository siting of all forms of nuclear waste has been mistakenly interpreted by much of the public as an inability to solve the waste issue.

Key recommendations for a national policy on nuclear waste management are as follows:

(1) The federal government must reaffirm a positive commitment to nuclear energy and support this commitment by the establishment and implementation of a well-defined waste management program.

(2) The federal government should assume broad responsibility for all aspects of nuclear waste management. This is necessary and desirable because of the broad national and international implications, the commonality of many aspects of defense- and civilian-generated waste problems, and the longevity of the hazards which may exceed the lifetime of many local government and civilian institutions.

Specific areas of responsibility may be delegated to industry and the states but with regulatory authority retained by the federal government.

(3) A single federal agency should be designated for the overall management of nuclear wastes. This agency should also have the total responsibility of coordinating the roles of the many departments and agencies which impact the management of all classes of nuclear wastes. This applies particularly to environmental standards, regulations, transportation and inspection for both defense and civilian nuclear waste requirements.

(4) The nuclear waste management agency of the federal government should have a permanent and autonomous structure independent of short-term and special-interest political forces.

(5) The federal governement should retain specific responsibility for the following waste categories either directly or through regulatory action:

  (a) All high-level and transuranic wastes generated by government and commercial nuclear activities.

  (b) All low-level wastes generated by government operations, including both defense and non-defense activities.

  (c) All spent nuclear power reactor fuel removed from electric utility-owned sites for interim storage or permanent disposal. Spent fuel should be retained in retrievable storage until the reprocessing issue is resolved.

(6) The commercial nuclear power industry should retain responsibility for spent fuel and low-level wastes temporarily stored on site and at other locations on its properties.

Industry wide standards are needed to establish uniform on-site storage capabilities for new reactors and to maximize facilities at existing sites.

(7) The disposal of low-level wastes generated by the nuclear power industry and other civilian activities should remain under the control of the states subject to federal regulatory standards and guidelines and specific federal authorization of interstate (regional) compacts. Present state and local restrictions on the use of low-level burial sites has created a critical situation for the continuation of nuclear power generation. Unless regulatory compliance is violated, these restrictions are unjustified and should be removed.

(8) The policy on spent nuclear fuel reprocessing and recycling should be separated from the proliferation issue and resolved on the basis of economic costs, benefits, and environmental impact.

(9) Continued acceptance of spent fuel from foreign countries for interim storage, disposal or reprocessing should be minimized until a decision is made to resume reprocessing and adequate interim away-from-reactor (AFR) storage and ultimate high level waste (HLW) disposal facilities are available.

(10) Civilian and defense waste issues should be integrated into a single system subject to the limitations of legitimate security considerations. This applies particularly to technology transfer and to final disposal facilities.

(11) A national plan for nuclear waste management must include effective state and public consultation processes. Scientific and engineering organizations should participate and play a leading role in these processes. The establishment in each state of a Planning Council on Radioactive Waste Management is endorsed.

The U.S. Congress must play a key role in removing the obstacles and defining the structure for a national policy and program for nuclear waste. A strong effort was made to launch a program based on the "Report to the President by the Interagency Review Group on Nuclear Waste Management", March 1979. A comprehensive legislative package equivalent to the Atomic Energy Act of 1954 is required. This legislation is urgently needed to and would provide for the effective and timely resolution of the issues listed above. A proposed program for key actions is as follows:

a. Authorize a Federal Government managed system for (AFR) spent fuel storage adequate to support continued operations of domestic reactors. One or more facilities should be available by 1984 or sooner. Plans should include the acquisition of one or more of the three closed, commercially owned reprocessing sites which have expandable storage capability.

b. Authorize the demonstration of a high-level waste (HLW) disposal systems, including processing, transportation and mined geological burial. These should be completed and operational by 1985.

c. Authorize the planning and design of a full-scale HLW repository to be started concurrently with the demonstration repository. Construction should begin as soon as possible after acceptable operation of the demo system, and at least one permanent, full size repository should be operational by the mid- or late 1990's. This repository should also be suitable for disposal of transuranic (TRU) waste and spent fule. However, spent fuel should be retained in retrievable storage until the reprocessing issue is resolved.

d. Specific conditions for the reprocessing of spent nuclear fuel should be determined. These conditions should include the construction and successful operation (by 1990) of a demonstration facility, which would integrate at a single location, reprocessing, spent fuel storage, mixed oxide (MOX), fuel fabrication, HLW liquid waste storage and its solidification and packaging for ultimate disposal. The government acquisition and adptation of the Allied General Nuclear Service Facility at Barnwell, S.C., should be considered. This assessment will determine whether or not commercial reprocessing and recycling should be reestablish.

e. A cooperative program between the states and the Federal government should be instituted to remove the restrictions on use of existing burial sites for low-level wastes (LLW) and to facilitate the opening of new sites. Legislation should define the authority of the Federal and state governments and provide authority for states to enter into agreements for regional sites.

f. Guidelines for decontamination and decommissioning of power plants, as well as other facilities of the nuclear fuel cycle that have reached the end of life, should be established. Major consideration should be given to problems arising from accidents which result in major equipment contamination.

g. A uniform national code should be established for the transportation requirements of radioactive material and nuclear wastes, including highway and rail routing and security controls. States should play an active role in developing such a code, but the responsible federal agency should have the ultimate authority for regulation of interstate shipments and for utilization of Federal government land for storage and repository sites.

APPENDIX A - SUPPORTING COMMENTS

Increased electrical energy production--based on nuclear fission and coal combustion--coupled with effective programs for energy conservations offer the most viable means for satifying a large portion of the nation's energy requirements for for at least the next fifty years. The elimination of oil for central station power generation and substitution of electrical energy for oil where economically feasible in water and space heating for industrial processes could save the equivalent of 70 to 80 percent of our current oil imports. Even optimistic estimates for coal production and the development of alternate energy sources indicate that the nuclear share must increase substantially to achieve a rate of growth of electric generation necessary to sustain a healthy economy and reduce dependence on imported oil. By the year 2000 twenty-five to thirty percent (300 to 360 gigawatts) of the electric power base should be nuclear-fueled (light water fission reactor) generation.

In the longer range (beyond 2010) serious and possibly insolvable problems concerning the environmental impact of large-scale coal combustion may preclude dependence on coal as the primary source for electric energy production. Advanced nuclear systems based on breeder technology offer the only long-term option for bulk generation of electric energy which has a high degree of technical and economic feasibility at the present time.

Nuclear energy must be an essential element of our national energy policy. Its continuing growth is vital to national security and public welfare. A major constraint to the aggressive utilization of nuclear power is political indecisiveness, rather than lack of technical and economic feasibility. Public concern over the perceived risk of radioactive waste release into the environment and weapons proliferation has had a significant influence. This perception has been stimulated and distorted by anti-nuclear activist groups, biased coverage by the news media and, to some extent, government and industry actions and policies. No single aspect of nuclear energy has generated as much public reaction as the failure of the government and industry to establish a coherent policy workable program for the management of nuclear wastes and particularly their ultimate disposal.

The objective of nuclear waste management is the isolation of existing and future radioactive wastes from the biosphere at all phases of the nuclear fuel cycle. As a general rule this isolation should be adequate to reduce public exposure under all conditions to levels which will have negligible effects, comparable or lower than those occurring naturally. A national policy and systems approach which will achieve a technically, politically and socially acceptable solution is urgently needed. A cooperative effort rather than an adversary relation among government, industry, the scientific and engineering community and the public is essential. Failure to develop and implement a solution will continue to stymie development of nuclear-fueled power and leave a dangergous legacy for future generations.

Some of the key issues and aspects of nuclear waste managment are as follows:

Historical perspective

Since the beginning of nuclear military programs in the early '40s and the beginning of commercial nuclear energy nearly 20 years later, radioactive wastes have accumulated to the present inventory of over 100 million cubic feet, which contains about 2 billion curies of radioactivity. About 10% of the volume and more that 98% of the radioactivity is contained in the high-level radioactive (fission product) wastes (HLW) generated in the fuel elements of commercial and defense reactors. A large quantity of relatively dilute but potentially hazardous alpha-emitting transuranic (TRU) waste has been generated in the production of nuclear weapons and in the reprocessing of naval nuclear fuel. Commercial TRU wastes which are now insignificant would become a significant waste product of reprocessing and recycling for both light water reactors (LWR) and breeder reactors.

There are about 10 million cubic feet of high-level military wastes stored above ground at government reservations. In additions, there are some 15 million cubic feet of buried and stored transuranic waste accumulated from AEC and DOE reprocessing and weapons fabrication also stored at government sites.

Most of the commercial high-level wastes are retained in spent fuel stored in water pools at utility-owned reactor sites. An additional smaller quantity of spent fuel and high-level liquid wastes from reprocessing are temporarily stored on commercial sites at West Valley, N.Y., and Morris, IL. The total spent fuel in interim storage is about 7800 metric tons (100×10^3 cu ft) which is less than 0.5% of the quantity of accumulated military high-level and TRU wastes. Because of the much higher fission-product content of commerical high-level waste, it has half of the total accumulated radioactivity. The indefinite deferral of reprocessing and the unavailability of away-from-reactor storage will require on-site storage of all reactor spent fuel until decisions are made and implemented.

Since no permanent geological repositories are now available for high-level wastes, whether from reprocessing or retained in spent fuel, they are stored on the surface of the earth. The containment of radioactive wastes from the environment at all phases of the fuel cycle has depended on engineered barriers, geographical separation and institutional controls. To date this system has been successful in limiting the radiation exposure of the public, even in areas of close to fuel-cycle operations, to far less than the natural background. A few accidents have occurred. Some of these, notably the Three Mile Island incident, and the leaking storage tanks at Hanford, have partially breached containment barriers; but in no known case has there been any serious environmental impact or radiation injury to the public.

Low-level wastes (LLW), which contain less than 10 nano curies per gram, are radioactivity contaminated solids, liquids, and gases generated in all phases of the nuclear fuel cycle and from research and medical activities. These wastes require little or no radioactive shielding but do require air and watertight containment to prevent environmental contalmination. The technology for processing, packaging and disposal is adequate and, if properly followed, will result in insignificant environmental impact. Lack of uniform procedures for managing the many forms of LLW and complex institutional factors are, however, major areas of concern. The large and increasing quantity of LLW (now about 70×10^6 cu ft), the limited number of burial facilities, and growing resistance of the states to intrastate transporting and disposal of any kind of nuclear waste, are creating a serious problem which needs to be resolved.

Historial perspective (continued)

A large quantity, about 150 x 10^6 tons of uranium mine and mill tailings from the front end of the fuel cycle, containing low concentrations of naturally occurring radioactive materials, have accumulated at the mining and milling locations. To date the environmental impact has been insignificant primarily because of the remote location. Increasing accumulations and the potential for wind-blown dispersion of radioactive gas and dust into the atmosphere, and the leaching of water soluable radioactive components in the earth, will require more than geographic isolation.

It is estimated that nearly two billion dollars has been spent by the federal government for nuclear waste management including R&D facility construction and operating costs. A substantial additional amount has been spent by the electric power industry for spent-fuel and low-level waste management.

The responsibility for government programs was assigned to a series of Federal Agencies (AEC, ERDA and DOE) through the Atomic Energy Act of 1954, the Energy Reorganization Act of 1974 and the Department of Energy Organization Act of 1977. This effort was directed to solving specific problems and resulted in the establishment of a strong technical capability without an overall national policy. In spite of the genuine effort of the Carter Administration to launch a national nuclear waste management policy, attempts in the Congress to provide the implementation legislation have so far been unsuccessful.

Technical Issues

A substantial technology base exists for the safe and reliable management of all forms of nuclear wastes. No scientific breakthroughs are required to develop and implement a total system which will isolate these wastes from the biosphere. However, to achieve a practial, cost-effective and reliable overall system, additional engineering development and experimental demonstrations are required in the following areas:

1. Solidification and packaging of high-level liquid wastes in forms that will satisfy criteria for long-term containment integrity, either in retrievable storage or ultimate disposal. This applies to both high-level defense wastes and much more radioactive fission product wastes derived from spent fuel.

2. Procedures and techniques for consolidation and storage of spent fuel elements in away-from-reactor (AFR) sites after initial thermal and radioactive cooling in water pools at reactor sites.

3. Characterization, site selection, and development and design of geologically stable, mined repositories including monitoring and security systems required during the operational phase.

Technical Issues (continued)

4. The reprocessing of commercial spent fuel, and the recycling of recovered plutonium and uranium will introduce additional technical problems including those concerning nuclear waste. These have not yet been adequately resolved, although a substantial technology base does exist from both US and foreign experience.

5. Systems analysis and synthesis that will integrate all phase of the waste management process to achieve a reasonably optimum overall solution.

6. Decommissioning and decontamination of reactors and waste management facilities particularly in the case of accident-damaged equipment, i.e., TMI, may present special technical problems.

7. Transportation, test and analysis of existing cask designs have demonstated containment integrity under all credible accident conditions, but additional testing may be required, as well as evaluations of special train service and routing strategies to increase public confidence.

Political and Institutional Issues

The political and institutional issues which must be resolved to achieve an effective national program of waste management are equally important and more intractable than technical issues and problems.

The establishment of policy and the management of high-level and transuranic wastes from both commercial and defense operations have been assigned by law to the Federal Governement. The Federal Government is also responsible for low-level wastes generated by government operations. The Department of Energy (DOE), and specifically the Office of Nuclear Waste Management, has primary responsibility for non-regulatory aspects and for interacting with regulatory programs. The functioning of this assignment is complicated by the division of power between legislative and executive branches of government in establishing basic policies and budgets and by the direct and indirect roles of other departments including Defense, Transportation, Interior and other agencies (NRC,EPA). Further complications arise from questions of state, Indian nation, and local government rights concerning transportation, storage and disposal of these wastes.

The states and the source organizations have authority for the management of commercial low-level wastes transported and disposed of within their own boundaries. This responsibility is subject to EPA and NRC guidelines and regulations. It is alos complicated by questions concerning local land use restrictions, interstate transportation regulations and regional disposal siting on federal land involving cooperative action among several states and the federal government.

Political and Institutional Issues (continued)

Nearly all free world foreign countries which have activies programs in nuclear energy also have focused national plans and firm commitments for commercial HLW management, including solidification, storage and geological disposal. Significant reprocessing of commercial spent fuel has been underway in other countries (France and the United Kingdom). In spite of the US moratorium, these countries, as well as Japan' and West Germany, plan to continue with reprocessing and do not perceive it to be a significant factor in weapons proliferation. The monolithic structure of the electric power industry under government management in these countries has simplified the planning and decision-making processes. The hope of constraining nuclear weapons proliferation on a worldwide basis by technical fixes of the fuel cycle and unilateral US political actions (reprocessing ban, breeder slowdown, etc.) is illusory. US policy generally, and the Nuclear Proliferation Act of 1978 in particular, may have effects opposite to those intended by inducing other countries to develop indigenous enrichment and reprocessing capabilities. A serious deterrent to the establishment of an acceptable national policy and action plan for nuclear waste is lack of enlighted public understanding of the nuclear waste problem. Unfortunately, the trend has been towards increasing concern and even hostility. This has occurred in spite of the fact that the record of both commercial and naval nuclear power has demonstrated the capability of the present imperfect system to isolate nuclear wastes safely. The reasons are complex and often emotional, rather than based on hard technical rationale and actual experience. Some of the key institutional factors which have confused the public and frustrated the enactment by Congress of a viable long-range policy are:

A. The implicit association of any problem in a nuclear power plant with the threat of radioactivity release. This has been stimulated by the news media coverage, as well as the propaganda of anti-nuclear groups. Partial failure of the industry to live up to early promises of reliability and availability have contributed to the lack of public confidence.

B. Failure of the nuclear industry, the federal government, and the supporting technical community to communicate effectively with the public. Over-zealous attempts to simplify and play down the total waste problem have exacerbated the situation.

C. Separation of civilian and military nuclear waste problems. It is believed that most of the public correctly perceives no fundamental distinction between civilian and military nuclear waste. Continued failure to establish an institutional framework to coordinate these sources will inhibit an overall systems solution.

D. Changes in Federal Executive Branch policy. The recent administration added to the public confusion and distrust by downplaying the historic role of the Federal Government as a strong supporter of nuclear energy growth to one of cautious acceptance as a short-term option.

E. The US moratorium on reprocessing and the slowdown of breeder reactor development based on largely unjustified linkage to weapons proliferation and potential increased radioactive waste problems. These actions have had a negative influence on the US public's perception of nuclear energy but have had little or no effect on reducing the worldwide risks of proliferation and nuclear waste hazards.

The body of literature on nuclear waste management and associated topics is extensive. The following list of references, while only a small part of the total, is believed to cover adequately the status and essential issues of the subject. The IEEE Energy Committee position is based on a review and analysis of this material and discussions with people knowledgeable in the field.

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Mr. HALL. That concludes the presentation of the panel. At this time we can have questions from the members.

The gentleman from Pennsylvania?

Mr. WALKER. Mr. Chairman, could I ask unanimous consent that an opening statement on my behalf be put at the appropriate place in the record of the hearing today?

Mr. HALL. Without objection.

Mr. WALKER. Thank you.

[The opening statement of Robert S. Walker follows:]

OPENING STATEMENT OF ROBERT S. WALKER

Thank you, Madam Chairman, I also wish to extend a welcome to our witnesses this afternoon on Magnetic Fusion. All of these people have participated in the magnetic fusion program under a variety of circumstances, some of them practically from the inception of the program over 20 years ago.

The magnetic fusion program exemplifies the kind of activity in which I believe the federal government has a strong responsibility for funding. We anticipate a long period of time for successful development of the Technology, We also are aware of the risks that are involved in carrying such a concept forward to eventual commercialization. On the other hand, we also see a large potential pay-out upon the success of the technology. This effort will certainly require the best of our nation's scientific community. It will also require a strong coordinated effort among the national laboratories, our colleges and universities, and the industrial sector. This committee has long recognized the interrelationships among these entities.

Our testimony today is somewhat unsettling in that certain strong charges are made by some of our witnesses. Specifically, we are advised that industry has not been called on to the extent that the industry feels itself to be capable. We get the same kind of claim by a university representative relative to our educational institutions. It would indeed be unfortunate if we did not utilize these two mainstays of our national economy and technology to their fullest extent. We will want to evaluate these claims more fully.

Madam Chairman, thank you for this opportunity to mention these interests. I am certain this hearing will be helpful.

Mr. WALKER. I have no questions at this time, Mr. Chairman.

Mr. HALL. We will go to Mr. Matson, I suppose. I will take you in the order that you presented your testimony. Inasmuch as I was not here for some of your presentation, I may inquire about something that you very adequately covered.

Would you comment on the role of the Engineering Design Center at Oak Ridge in the context of the role of a Center for Fusion Engineering?

Mr. **Matson**. I think the Design Center as we see it now seems to be functioning and acting more as an analysis center as opposed to a design center. I think, as the Act contemplated, a CFE would be a situation where you would have a focus centered on true engineering development being done in concert with the science and technology program. I think DOE's view currently is, that the engineering need is not there now. We fail to see that to be true.

I think you cannot have technology going along and, all of a sudden, discontinue and startup engineering. They have to be done in concert. I think others on the panel here have said that in order for an industry data base to be developed, industry has to be involved very early on. It may not have to be at the highest level, as the technology program is proceeding, but there has to be involvement of industry. We do not see that at this point at Oak Ridge.

Mr. **Hall**. Would you like maybe to enlarge on the specific priorities that you would recommend, for program funding enhancements in fiscal year 1984, for the Fusion Materials Irradiation Facility and other facilities?

Mr. **Matson**. I do not feel qualified to give you a real qualitative answer on specific facilities. Albeit, I think we see—and I did mention in my testimony—it would be very important for dollars to be set aside, and we mentioned the figure of $1 million or so. The amount would be at the committee's option. This would be to, in fact, bring together a start toward an engineering development plan that could move in concert with the technology development plan that DOE has established.

I have in front of me an item from The Commerce Business Daily of March 3, 1983, where DOE has put out or called for interest in a fusion reactor design project management support services. There is likely to be an RFP, as we see it, that will come out of DOE for this. They will be looking for involvement with industry to give some very specific input as to being able to plan, design, develop, implement, and provide background about large, complex, hightech projects.

I think we can applaud this kind of effort as a start, but we need considerebly more in that direction to get industry involved.

Mr. **Hall**. Dr. Agnew, you seem to be the one most nearly satisfied with the budget. Do you want to comment?

Dr. **Agnew**. Mr. Chairman, I think I would be remiss in the context of one of the speaker's remarks if I did not point out, having come from a national laboratory/university sponsored organization, that we at GA are private industry; a wholly owned subsidiary of Gulf Oil. We started the fusion program with Texas Utilities many years ago under the leadership of Dr. Ohkawa. We have spent probably more than $30 million of our own money. The buildings at GA, of course, are GA buildings, not Government facilities.

Our private work in OHTE will cost some $20 million in private funds. I have mixed feelings. I certainly support what Dr. Mense said with regard to involving industry, but in our case we are industry, having learned for the first time in our life something called profit and loss and cash flows compared with the national lab-type budget.

We are involving the various suppliers. We, ourselves, are supplying materials to Princeton on limiters. We are working on particular types of power tubes.

I certainly agree with what he says, but I would like to make it clear to the committee that we view ourselves as private industry working in partnership with the Government, using the basic generic programs of the Government in order to develop programs.

As I mentioned in my testimony, there are two things that I did think should be added on. One was the tokamak fusion core demonstration device and the other the national ion cyclotron heating engineering program.

However, there is another point I think that perhaps we should raise. I do not know where in the Congress it should be raised. That is, worldwide the fusion community is going to need tritium, and they are going to need it for a good number of years, until they can breed their own.

If you and your colleagues are worried about proliferation, I think it would be very prudent on the part of the U.S. Government to announce that they would be a reliable supplier of tritium to the free world's fusion program, until that program is able to breed its own material. If you do not do that, it will certainly lend the opportunity for many nations that have a fusion program to build the type of devices which are required to make tritium, namely, fission devices, which can be used to make other weapons materials. I think if the United States would take the initiative and say that we will be a supplier to the world's fusion program, that could be a very big plus to the community and as a nonproliferation issue.

I do not know where this should originate, but if there are funds down the road to be spent, that could be a program.

I do not know where the tritium is going to come from for our own program, which will take a considerable amount, or for the world's program.

Mr. HALL. Thank you, Dr. Agnew.

I might ask Mr. McCormack, with his background and his obvious knowledge of the subject matter, what he thinks the chances today would be for that type of overwhelming support from the Congress, if we had the Magnetic Fusion Energy Engineering Act of 1983 presented.

Mr. MCCORMACK. Is your question, Mr. Chairman, would the bill be passed today, with a $20 billion price tag on it?

Mr. HALL. Would it be passed this afternoon? I think that is a more timely question.

Mr. MCCORMACK. Mr. Chairman, Dr. Mense and I, supported by the gentlemen at this table and many other men and women from all across the country, spent several years educating the Congress about the program before we presented the bill for enactment. I do not think you could pass any $20 billion program of any sort in this Congress this afternoon without rather extensive preparation.

However, I believe that if a program of information were established, as we did in 1979 and 1980 to inform the Members of Congress about the fusion program, then I believe they would support it.

Mr. HALL. However, you would doubt seriously that we would have that educational program in place today, don't you?

Mr. McCormack. I beg your pardon?

Mr. Hall. You doubt seriously that we have that educational program in place today?

Mr. McCormack. Yes. It is quite obvious that we do not.

Mr. Hall. I noted that it passed almost without dissent.

Mr. McCormack. Yes. Passage was unanimous in the Senate, and there were seven dissenting votes in the House. It was passed under suspension of the rules, as a matter of fact. It had very strong bipartisan support.

Mr. Hall. Does anyone have any other questions?

Mr. McCormack. Mr. Chairman, may I please make one additional point?

Mr. Hall. Yes, sir.

Mr. McCormack. Thank you. Mr. Chairman, every witness at this table today has stressed the importance of industrial involvement in the fusion program. Each of us prepared our testimony individually. I am confident that no two of us talked to any of the others about our testimony. However, each of us came here with the same message, and it is that the fusion program has a purpose, and that purpose now requires getting industry involved in the engineering development and materials testing program, so that we can move forward toward commercialization, and away from pure research. Each of us came to that conclusion independently, and each expressed it in his own way.

Mr. Hall. I surely agree with that. I recognize the backgrounds, the very impressive backgrounds, that have been presented in this panel—with practical experience of years on the floor; Dr. Mense's experience helping to staff the bill I believe, for this committee. Is that right?

Mr. McCormack. I should say that we could not have passed the bill in 1980 without Al Mense.

Mr. Hall. I believe you all might have gotten together on that one. [Laughter.]

Dr. Mense. Mr. Chairman, I will modestly accept the compliments. [Laughter.]

Mr. Hall. I might say, in a moment of levity, that your story was interesting. For any of us who have bought a car recently, we could say, "I was that boy." [Laughter.]

I thank you gentlemen. If you have anything prepared that you want to submit for the record, please do so.

We will again express our appreciation with assurance, as Dr. Mense well knows, as does former Congressman McCormack, that your testimony will be read and reread by staff and other members. Their failure to appear here does not indicate any lack of interest or knowledge of the importance of your testimony or the dignity of your presence.

Thank you for coming.

Let's get panel No. 2 underway.

Is Mr. Dawson in the crowd?

A Voice. He was a while ago.

Mr. Hall. OK. We will stand by for a moment or so. He will be back. Mr. Dawson surely will be arriving shortly.

In the interest of time, let's go ahead.

Dr. Shohet, in your presentation, as I understand it, you are going to be wearing two hats today—one as chairman and one as professor of the University of Wisconsin? Two hats, three hats, whatever.

We look forward to your testimony. You might just generally give us your testimony, if you wish, or speak from your prepared text. However, whatever you do, if you would, please let the court reporter have copies for the record. They will go into the record in their entirety. You may handle it any way you wish to handle it, whatever you feel the most comfortable with, Doctor.

STATEMENT OF DR. J. LEON SHOHET, PROFESSOR AND DIRECTOR, TORSATRON/STELLARATOR LABORATORY, UNIVERSITY OF WISCONSIN, CHAIRMAN, PLASMA COORDINATING COMMITTEE AND CHAIRMAN, UNIVERSITY FUSION ASSOCIATION

Dr. SHOHET. Thank you, Mr. Chairman.

I am Leon Shohet, and I am professor of electrical and computer engineering at the University of Wisconsin. I am also the director of the university's torsatron/stellarator laboratory and am the chairman of its Plasma Coordinating Committee.

I am also speaking to you today on behalf of the University Fusion Association, of which I am the chairman. This association was established in 1979, and it is an organization of faculty and researchers whose members belong to 50 universities in the United States, all working in the area of fusion.

It is my pleasure to appear before you and make a brief statement on behalf of both of these institutions.

We appreciate your efforts on behalf of the fusion community of the Nation's universities because we know that the task of authorizing the fusion budget is a very difficult one.

For the record, I have submitted two statements. I wonder if they both could be accepted in the record.

Mr. HALL. Without objection, they will be included following your oral testimony.

Dr. SHOHET. I shall first begin with the University Fusion Association. We believe that it is important for you to gain some perspective on the university role in the fusion program. From this I think you will see just what we perceive to be the problems and opportunities for universities in fusion.

I should point out to you at the outset that this testimony represents a summary of the opinions of many members of the University Fusion Association executive committee. I was impressed by the fact that the general tone of all of the comments has been very similar; that is, it will be very difficult for universities to continue their commitment to the fusion program because of the lack of adequate vehicles and financial resources. It is our hope that you will assist us in contributing toward the goal we all seek—commercial fusion power.

As you well know, the budget for fusion has grown considerably from its early years. It is important to note that a similar growth in the university activity in fusion has not taken place. For fiscal year 1984, DOE proposes an allocation to universities of slightly more than 4 percent of the total fusion budget, $21 million. We think that over the years the fraction of funds devoted to universities has decreased because of the shifting emphasis from research to relying on only a few options in the program along with rigid direction from DOE.

The ever-increasing programmatics have caused a number of university programs to be squeezed out, have their budgets substantially cut, or new proposals not to be funded. It is folly to terminate a $50,000 to $100,000 a year university program to buy another pump for a beam line on a major machine at a national laboratory.

Since the bulk of the funding for fusion goes to the national laboratories, we do know that some of this funding does get "passed through" to industries and universities. However, three problems occur with this arrangement.

First, laboratories often charge a fee for this passthrough, sometimes as much as 20 percent.

Second, when budgets get tight, the first thing that gets canceled are the outside funding projects.

Third, the prime contractor is the national lab and not the university or industry, so the program is directed downward to the researcher, rather than from the researcher upward. Thus, it appears that current DOE policy is that both universities and industry act only to service the needs of the national laboratories.

This, in our opinion, is dangerously wrong. The goal of fusion research is to make fusion, not to support a group of national laboratories.

Also, it is clear that the fusion problem will not be solved by this generation of researchers, and, at the very least, we must recognize that universities are the only source of newly trained manpower for fusion and can only continue to attract "the best and the brightest" if the scope of the programs are meaningful.

In addition, university activities broaden the scope of the fusion program. They provide new ideas and are more cost-effective and more flexible than the national laboratories and industry. One of the major problems we see today is that, in fact, new ideas cannot be fully developed at universities. One often gets the impression that not only do national laboratories want to do the major programmatic experiments, they want to do all the experiments. Several examples can be given of programs at national laboratories which are of the scale and scope of those normally thought to be done at universities.

We are, nevertheless, supportive of university industry cooperation with the national laboratories. However, except for a very few exceptions, my frank observation, covering a period of over 20 years, is that there has been very little meaningful interaction.

National laboratory staff members do not seem to appreciate that a principal reason for doing research on campus is to establish and maintain a research environment into which new students can continually feed. Another reason for the problem is that national laboratory staff members generally feel that they do not need university participation. I think this results, in part, from feelings of the national laboratories and the DOE Office of Fusion Energy that university staff members and students cannot get things done on schedule.

Another argument frequently used by DOE and those who argue in favor of having the research done at a national laboratory is that many of the programs are large and multidisciplinary and, thus, unsuited for a university. These arguments have weight.

However, more than one example can be cited of large, multidisciplinary, federally funded fusion research programs carried on successfully by universities, when funding has been provided on a firm basis for some reasonable period of time.

In the U.S. fusion program, the TEXT tokamak, which has an approximate construction cost of $10 million, at the University of Texas; the Phaedrus experiment at the University of Wisconsin, with a similar price tag; and the TARA experiment at MIT, at an estimated cost of $18 million, are superb examples of the level and type of program that can be managed at universities in fusion.

In Japan, the $35 million heliotron-E stellarator, built jointly with industry, is now operated at Kyoto University by a staff of under a dozen people that includes several graduate students with a professor of electrical engineering in charge.

All of these programs are very small compared to national laboratory activities, but all have contributed far out of proportion to their cost.

UFA is confident that strong, productive, and cooperative research programs between the national laboratories, universities, and industry can be done. University overhead rates are significantly lower than those of national laboratories. They do not have, nor require, large, in-house construction and design groups, since much of what is done can be designed in cooperation with industry.

It is also most important to again note that this research makes a significant contribution toward the training of the next generation of scientists and engineers. This constant flow of new personnel results in a stimulating environment for the development of new ideas and concepts.

Examples of this include: The rebirth of stellarators in the United States, the importance of beta for fusion reactors, three-dimensional computer codes and numerical simulation, high field tokamak operation, r.f. current drive, discharge cleaning, and major theoretical contributions to tokamaks and tandem mirrors.

Basically, we feel that DOE must provide a mechanism for all possible participants, either at national laboratories, industry, or universities, to develop and propose the most meaningful, cost-effective, and relevant ideas, rather than relegating industry and universities to a subservient role to the labs.

If the reviews are good and an institution is judged capable of carrying out the research, it should be funded. Nobody has a monopoly on good ideas. We need the best of them from all sectors to solve the fusion problem.

I would now like to turn to the presentation on behalf of the University of Wisconsin.

First, I must point out that the University of Wisconsin has been engaged in fusion research since 1962. Programs at Wisconsin have been characterized by important discoveries in experimental plasma research, innovative fusion reactor studies, and recently by concomitant theoretical work.

Wisconsin has always been interested in unique, flexible, and out-of-the-ordinary approaches and has at present what we perceive to be the greatest variety of experimental programs of any institution in the United States, as well as many foreign laboratories.

Twenty-five faculty members, 49 scientific and professional staff, and 90 graduate students make a total of 164 persons on the Madison campus that are directly participating in this program.

Experimental facilities include the tokapole/multipole laboratory, the Phaedrus tandem mirror experiment, and the torsatron/stellarator laboratory, as well as numerous smaller experiments.

In the written presentation table 1 shows the funding for Wisconsin from both DOE and NSF sources for fiscal years 1982 and 1983. The Wisconsin tradition is for the individual faculty member or group of faculty members to negotiate separately with the Department of Energy regarding the status of each experimental and theoretical program. We believe this permits the best interaction possible between each principal investigator and the Department of Energy.

In addition, a universitywide plasma coordinating committee, which is composed of faculty and scientific staff, meets several times per year regarding curriculum matters, joint organization for scientific meetings, and support for numerous individual efforts. The committee has basically operated in this format for the past 16 years and has been used as a model of interdepartmental cooperation and operation.

I would like to spend a minute or two talking about the experimental programs.

The tokapole/multipole experiments have been operating on the Madison campus for more than 20 years. Figure 1 in the handout shows a photograph of the tokapole device. You may be interested to note that the large octupole, also operated by this program, is the same size as the PLT tokamak which is at the Princeton Plasma Physics Laboratory and was, until last December, the largest operating tokamak in the United States.

The tokapole/multipole program was the first to demonstrate quiescent operation of plasmas that were thought to be unstable, along with operation at high plasma pressure. Table II in the handout shows a list of other achievements of this program.

The second figure shows a photograph of the Phaedrus tandem mirror experiment. This device, which is 32 feet long and 6 feet in diameter, has been the largest experiment constructed at Wisconsin. Its construction cost in today's dollars is approximately $10 million.

The Phaedrus experiment, which is currently the main backup experiment to those at the Lawrence Livermore Laboratory, was the first to demonstrate that RF heating will work on a tandem mirror and that, in fact, only RF was needed to produce a tandem mirror plasma. Table III in the handout shows a more extensive list of the Phaedrus program's achievements.

I now turn to the torsatron/stellarator laboratory. This laboratory began its operation in 1973 with the acquisition from the Culham laboratory in the United Kingdom of the Proto-Cleo stellarator. You may know that the Atomic Energy Commission abandoned stellarators in the United States in favor of tokamaks in

1969. As a result, there was no activity in stellarators in this country until Proto-Cleo arrived in Madison in 1974.

I must point out that the staff of the laboratory is most grateful to the National Science Foundation for providing the funds with which to purchase Proto-Cleo. It is my opinion that it took great courage for NSF to fulfill their faith in the Wisconsin researchers, especially since the Atomic Energy Commission had no wish to support such a program.

The torsatron/stellarator laboratory has had many achievements, including the first demonstration of Alfven wave heating and the first measurement of equilibrium and transport currents. In 1978 and 1979 the laboratory proposed the construction of two clearly innovative new stellarators: the Wistor and Wistor-U devices, the latter one of which is the basic design used for the ATF experiment just authorized by DOE to be built at Oak Ridge. In fact, we believe that the Wisconsin program in stellarators is the main reason that there has been such a strong rebirth of stellarator interest in the United States today.

I am also pleased to tell you that last Friday the university installed its latest stellarator, the IMS modular stellarator, which was designed and built entirely by the University of Wisconsin staff. It is the first modular stellarator in the world and, should it be successful in its initial operation, we expect that all future stellarators will be built using this principle.

The third figure in the handout shows a photograph of the IMS device after it won the grand prize of Design News magazine as the most innovative engineering design of 1982. Table IV shows a list of additional achievements of the torsatron/stellarator laboratory.

I now turn to the fusion engineering program. This program began in 1965 with experimental fusion technology studies and was commissioned to do the first full-scale reactor study in 1971. A Wisconsin team, often assisted by industry and national laboratories, has carried out major efforts in reactor studies during the last 12 years. Many different designs have been investigated, from tokamaks and stellarators to tandem mirrors to lasers and other inertial fusion devices.

To give you an idea of the scope of the program, 11 major fusion reactor studies have been completed, over 500 reports written with more than 89,000 copies distributed worldwide and over 230 articles in refereed journals have been published.

The fusion engineering program was the first to recognize the importance of beta as a figure of merit in fusion reactors and the first to coordinate a realistic complete costing study involving industry and the national laboratories. Table V shows some additional firsts for this program.

A comparatively new development at Wisconsin has been the institution of a plasma and fusion theory program that is, of itself, not directly connected with the experimental programs. The theory group's recent achievements are listed in table VI.

Thus, I think you can see that the program at Wisconsin has been one of the major ones in fusion research for a long time. Over 135 Ph. D. degrees have been awarded in fusion. The university is very proud of its leadership in this area.

Yet, with all of this behind us, we at Wisconsin are greatly troubled. Over the past few years, events have occurred which have given the university administration great cause for alarm regarding the intent of the Department of Energy in fusion, both at Wisconsin and at universities in general. I would appreciate sharing some of these concerns with you.

One, between the years 1982 and 1983, the fusion engineering program in systems studies has had its budget cut by nearly 50 percent and the bulk of its activities transferred to national laboratories.

Two, the tokapole/multipole laboratory has had severe budget cuts and is threatened with yearly extinction.

Three, the torsatron/stellarator laboratory has seen its favorably reviewed proposals unfunded and, instead, its ideas were given to an inexperienced group at a national laboratory. Last year, even though Congress had recognized this problem and directed the Department of Energy to provide Wisconsin with appropriate funding in fiscal year 1983, the laboratory has not received the funding but, instead, has been informed the DOE wishes to completely terminate the laboratory's activities.

This is the state of affairs that greets the University of Wisconsin's administration. Personally, I have been most grateful to the university for its willingness to go the extra mile and to undertake a massive effort on behalf of the fusion program at Wisconsin to insure that it will remain viable.

We believe that it is not sufficient merely to recite our problems but it is imperative that we propose solutions to you.

First, we believe that our problems are certainly not unique among the university fusion community. As far as the program at Wisconsin is concerned, we feel that the following must be done to sustain the program:

One, fund, as Congress has directed, the torsatron/stellarator laboratory at the level of $1.5 million in fiscal year 1983 and provide $2 million in fiscal year 1984 to continue its present research program and its design study, together with its three industrial partners, of the next generation experiment.

I would like for a moment to mention the relationship between our program and the ATF facility at Oak Ridge. We are not in competition with them. Rather, we are following the program of a subpanel on stellarators of the Magnetic Fusion Advisory Committee.

Our program has followed the recommendations of the panel which propose that Wisconsin would emphasize modular stellarators, not continuous coil devices, as at Oak Ridge. We will also be designing, as the panel recommended, a superconducting coil device not a copper coil device, as at Oak Ridge. In short, our program is fully compatible and based on the assumption that ATF will be built at Oak Ridge.

No. 2, restore the funding level for the system studies program at Wisconsin to fiscal year 1982 level, which will permit this program to retain its well-earned right to continue providing independent and thorough reactor analyses of fusion concepts as well as to continue to provide the training of fusion engineers with a broad exposure to many aspects of the fusion problem, so that fusion can be carried to the commercialization stage.

Three, provide the fiscal year 1982 funding level for operation of the tokapole/multipole program.

Four, and perhaps most importantly, assure the university of Wisconsin administration that continued, long-term funding is expected so that the university can rely on its status as a major fusion research center and can continue to encourage faculty and staff to work in this area.

In summary, both we at Wisconsin and the University Fusion Association believe that DOE should follow the philosophy expressed in the following six points:

One, universities should be considered a national resource for research. Industry should be considered a national resource for product development.

Two, the present fusion budget provides only slightly more than 4 percent of its total to universities and has not kept pace with the scope and needs of the fusion program. This is an unconscionable loss of the great resources available in universities today.

No. 3, universities have contributed key ideas to the fusion program, but will no longer be able to do so unless they can be assured of continued, long-term funding and are not forced into a role of simply providing service to the national labs. Otherwise, key personel will be lost, competent research teams will be broken up, and university administrations will no longer encourage faculty to become involved in fusion.

Four, joint university-industry programs are both productive and very cost-effective.

Five, universities are the only source of newly trained manpower for fusion and can only continue to attract and train "the best and the brightest" if the scope of the programs are meaningful.

Six, we must provide a mechanism for all possible participants, either at national laboratories, industry or universities, to develop and propose the most meaningful, cost-effective, and relevant ideas, rather than relegating industry and universities to a subservient role to the labs. If the reviews are good and an institution is judged capable of carrying out the research, it should be funded. Nobody has a monopoly on good ideas, and we need the best of all of them from all sectors to solve the fusion problem.

Thank you very much.

[The prepared statement of Dr. Shohet follows:]

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STATEMENT OF

DR. J. LEON SHOHET, CHAIRMAN

UNIVERSITY OF WISCONSIN-MADISON PLASMA COORDINATING COMMITTEE

BEFORE THE

SUBCOMMITTEE ON

ENERGY RESEARCH AND PRODUCTION

HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

ON

THE DEPARTMENT OF ENERGY FY 1984 BUDGET

MARCH 16, 1983

SUMMARY

The University of Wisconsin urges the Subcommittee on Energy Research of the House Committee on Science and Technology to consider the following points in authorizing the 1984 DOE budget.

1. 164 persons on the Madison Campus of the University of Wisconsin currently are participating in the fusion program.
2. 3 major experimental programs -- Tokapole/Multipole, Phaedrus Tandem Mirror and Torsatron/Stellarator comprise the Wisconsin effort along with the Fusion Engineering and Theory Programs.
3. The University is seriously troubled about its future in this area, in the wake of severe budget cuts in the tokapole/multipole and system studies programs, and an impending termination of the torsatron/stellarator program even after Congress has directed that DOE provide $1.5 million in FY 83 for the latter.
4. We urge restoration of the budget cuts for the Tokapole/Multipole and Fusion Engineering programs and full funding of $1.5 million in FY 83 and $2.0 million in FY 84 for the Torsatron/Stellarator Program.
5. This action will permit the University to continue its well-earned major role in fusion research, and to continue to provide broadly trained plasma and fusion scientists and engineers as well as to fully develop its innovative and unique ideas.

I wish to express my appreciation for providing me with the additional opportunity to appear before you as a representative of the University of Wisconsin. For the record, I am Leon Shohet, and I am a professor in the College of Engineering at the University of Wisconsin. I am the director of the Torsatron/Stellarator Laboratory, and I am the Chairman of the University's Plasma Coordinating Committee.

The University of Wisconsin has been engaged in fusion research since 1962 when Professor Donald Kerst returned to the Physics Department. Programs at Wisconsin have been characterized by important discoveries in experimental plasma research, innovative fusion reactor studies, and recently by concommitant theoretical work. Wisconsin has always been interested in unique, flexible and out-of-the ordinary approaches, and has, at present, the greatest variety of experimental programs of any institution in the U.S., as well as many foreign laboratories.

Twenty-five faculty members, 49 scientific and professional staff and 90 graduate students, make a total of 164 persons on the Madison Campus that are directly participating in this program.

Experimental facilities include the Tokapole/Multipole Laboratory, the Phaedrus Tandem Mirror Experiment, and the Torsatron/Stellarator Laboratory, as well as numerous smaller experimental devices. Table I shows the present funding for Wisconsin from both Department of Energy and National Science Foundation sources for Fiscal Years 1982 and 1983. The Wisconsin tradition is for the individual faculty member or group of faculty members to negotiate separately with the Department of Energy regarding the status of each experimental and theoretical program. We believe this permits the best interaction possible between each Principal Investigator and the Department of Energy. In addition, a University-wide plasma coordinating committee, which is composed of faculty and scientific staff, meets several times per year regarding curriculum matters, joint organization for scientific meetings, and support for numerous individual efforts. The committee has operated in basically this format for the past 16 years, and has been used as a model of interdepartmental cooperation and operation.

I now turn to a brief description of the major experimental programs. The tokapole-multipole experiments have been operating on the Madison Campus for more than 20 years. Figure 1 shows a photograph of the tokapole device. You may be interested to note that the large octupole is the same size as the PLT tokamak which is at the Plasma Physics Laboratory at Princeton, and was, until last December, the largest operating tokamak in the United States. The tokapole/multipole program was the first to demonstrate quiescent operation of plasmas that were thought to be unstable, along with operation at high plasma pressure. Table II shows a list of other major achievements of the tokapole/multipole program.

Figure 2 shows a photograph of the Phaedrus tandem mirror experiment. This device, which is 32 feet long and 6 feet in diameter, has been the largest experiment constructed at Wisconsin. Its construction cost in today's dollars is approximately $10 million. The Phaedrus experiment was notably the first to demonstrate that RF heating will work on a tandem mirror and, in fact, that only RF was needed to produce a tandem mirror plasma. It also was the first to demonstrate stability limits in a tandem mirror. Table III shows a more extensive list of the Phaedrus program's achievements.

I now turn to the Torsatron/Stellarator Laboratory. This laboratory began its operation in 1973, with the acquisition, from the Culham Laboratory, in the United Kingdom, of the Proto-Cleo stellarator. You may know that the Atomic Energy Commission abandoned stellarators in favor of tokamaks in 1969. As a result, there was no activity in stellarators in the U.S. until Proto-Cleo arrived in Madison in 1974. I must point out that the staff of the laboratory is most grateful to the National Science Foundation for providing the funds with which to purchase Proto-Cleo. It took a considerable effort on their part, especially since the Atomic Energy Commission had no wish to support such a program.

The Torsatron/Stellarator Laboratory has had many achievements, including the first demonstration of Alfvén wave heating and the first measurements of equilibrium and transport currents. In 1978 and 1979, the laboratory proposed the construction of two clearly innovative new stellarators: the WISTOR and WISTOR-U devices, the latter one of which is the basic design used for the ATF experiment, just authorized by the Department of Energy to be built at Oak Ridge. In fact, we believe that the Wisconsin program in stellarators is the main reason that there has been such a strong rebirth of stellarator interest in the United States today.

I am also pleased to tell you that last Friday, the University installed its latest stellarator, the IMS Modular Stellarator, which was designed and built entirely by the University of Wisconsin staff. It is the first modular stellarator in the world, and should it be successful in its initial operation, we expect that all future stellarators will be built using this principle. Figure 3 shows a photograph of the IMS device, after it won the Grand Prize of Design News Magazine for the most innovative engineering design of 1982. Table IV shows a list of additional achievements of the Torsatron/Stellarator Laboratory.

I now turn to the Fusion Engineering Program. This program began in 1965, with experimental fusion technology studies, and was commissioned to do the first full-scale reactor study in 1971. A Wisconsin team, often assisted by industry and national laboratories, has carried out major efforts in reactor studies during the last 12 years. Many different designs have been investigated, from tokamaks and stellarators to tandem mirrors to lasers and other inertial fusion devices. To give you an idea of the scope of this program, 11 major fusion reactor studies have been completed, over 500 reports written, with more than 89,000 copies distributed world-wide and over 230 articles in refereed journals have been published. The Fusion Engineering Program was the first to recognize the importance of beta as a figure of merit in fusion reactors, and the first to coordinate a realistic complete costing study involving industry, and the first materials testing reactor for fusion, along with 16 different detailed reactor related computer codes used by industry and national laboratories. Table V shows some additional "firsts" for this program.

A comparatively new development at Wisconsin has been the institution of a plasma and fusion theory program that is of itself, not directly connected with the experimental programs. This program's recent achievements are listed in Table VI.

Thus, I think you can see that the program at Wisconsin has been one of the major ones in fusion research for a long time. Over 135 Ph.D. degrees have been awarded in fusion. The University is very proud of its leadership in the fusion program.

Yet, with all of this behind us, we at Wisconsin are greatly troubled. Over the past few years, events have occurred which have given the University Administration great cause for alarm regarding the intent of the Department of Energy in fusion, both at Wisconsin, and at universities in general. I would appreciate sharing some of these concerns with you.

1. Between the years 1982 and 1983, the Fusion Engineering Program in systems studies, has had its budget cut by nearly 50 percent and the bulk of its activities transferred to national laboratories.

2. The Tokapole/Multipole Laboratory has had severe budget cuts and is threatened with yearly extinction.

3. The Torsatron/Stellarator Laboratory has seen its favorablly reviewed proposals unfunded, and instead, its ideas were given to an inexperienced group at a national laboratory. Last year, even though Congress had recognized this problem, and directed the Department of Energy to provide Wisconsin with appropriate funding in year 1983, the laboratory has not received the funding, but instead, has been informed that Department of Energy wishes to completely terminate the laboratory's activities.

This is the state of affairs that greets the University of Wisconsin's Administration. I, personally, have been most grateful to the University for its willingness to "go the extra mile" and to undertake a massive effort on behalf of the fusion program at Wisconsin to ensure that it will remain viable.

We also believe that it is not sufficient merely to recite our problems, but it is imperative that we propose solutions to you. First, we believe that our problems are certainly not unique among the University Fusion Community. Thus, we would like to subscribe to the general principles that were presented to you earlier this afternoon on behalf of the University Fusion Association. They are:

1. Universities should be considered a national resource for research; industry a national resource for product development.
2. The present fusion budget provides only slightly more that 4% of its total to universities and has not kept pace with the scope and needs of the fusion program. This is an unconscionable loss of the great resources available in universities today. The High Energy Physics Program of DOE is an example of an appropriate relationship between universities and national laboratories.
3. Universities have contributed key ideas to the fusion program, but will no longer be able to do so unless they can be assured of continued, long term funding and are not forced into a role of simply providing service to the national labs. Otherwise, key personnel will be lost, competent research teams will be broken up, and university administrations will no longer encourage faculty to become involved in fusion.
4. Joint university-industry programs are both productive and very cost productive.
5. Universities are the only source of newly trained manpower for fusion and can only continue to attract and train "the best and the brightest" if the scope of the programs are meaningful.
6. All possible participants should be able to compete fairly and to propose and develop their ideas without the built-in biases that currently exist in OFE. They should receive a fair hearing and funding if the research is deemed relevant, and if they are judged competent to perform the research.

As far as the program at Wisconsin is concerned, we feel that the following must be done to sustain the program:

1. Fund, as Congress has directed, the Torsatron/Stellarator Laboratory at the level of $1.5 million in FY 83 and provide $2.0 million in FY 1984 to continue its present research program and its design study, together with its 3 industrial partners, of the next generation experiment.
2. Restore the funding level for the system studies program at Wisconsin to the FY 82 level, which will permit this program to retain its well-earned right to continue providing independent and thorough reactor analyses of fusion concepts as well as to continue to provide the training of fusion engineers with a broad exposure to many aspects of the fusion problem, so that fusion can be carried to the commercialization stage.
3. Provide the FY 82 funding level for operation of the Tokapole/Multipole program.
4. Assure the University of Wisconsin Administration that continued long-term funding is expected so that the University can rely on its status as a major fusion research center and can continue to encourage faculty and staff to work in this area.

DR. J. LEON SHOHET

Dr. J. Leon Shohet is Professor of Electrical and Computer Engineering at the University of Wisconsin-Madison. He received his B.S. degree in electrical engineering from Purdue University in 1958 and the M.S. and Ph.D. degrees in electrical engineering from Carnegie-Mellon University in 1960 and 1961, respectively.

Honors include: Frederick Emmons Terman Award of the American Society for Engineering Education; Merit Award of the Nuclear and Plasma Sciences Society of the Institute of Electrical and Electronics Engineers; Fellow of the American Physical Society; Fellow of the Institute of Electrical and Electronics Engineers; Distinguished Student at Purdue University; Membership in Eta Kappa Nu and Tau Beta Pi.

Dr. Shohet is the author of a major textbook in the field of plasma and fusion science and has written a chapter on stellarators for a book on fusion edited by Edward Teller. He is the Chairman of the Coordinating Committee on Plasmas at the University of Wisconsin and director of the University's torsatron/stellarator laboratory. In addition, he is the author of over 200 publications on plasma physics, magnetic and inertial fusion, and biophysics.

He is a member of the U.S. Department of Energy stellarator advisory committee and also a member of the DOE-EURATOM steering committee on stellarators. He is a member of several professional organizations, including the Institute of Electrical and Electronics Engineers (in which he has served as president of the Nuclear and Plasma Sciences Society and a member of the IEEE Technical Activities Board and is currently the chairman of the IEEE Research and Development Committee), the American Physical Society and the American Association for the Advancement of Science. In 1973 Dr. Shohet founded the Institute for Electrical and Electronics Engineers publication Transactions on Plasma Science, which was the first engineering publication devoted to the area of plasma physics and fusion. Dr. shohet has served as consultant to numerous U.S. national laboratories and industries. he is currently Chairman of the university Fusion Association.

He is listed in Who's Who in the Midwest and in American Men and Women of Science.

TABLE I

DOE SUPPORTED PLASMA PHYSICS AND RELATED RESEARCH

Principal Investigator	Project	FY 82	FY 83
Hershkowitz, et. al.	Investigation of RH Heating for Tandem Mirror Expts. and Reactor	$1,000,000	$1,400,000
Shohet, et. al.	Study and Design of a Steady Superconducting Torsatron	450,000	315,000 (1,500,000 appropriated b Congress)
Larbalestier	Practical Superconducting Composite Conductors:Understanding, Optimization, and Development	40,000	65,000
Kulcinski, et. al.	Radiation Damage Studies for Fusion Reactors	147,000	93,000
Callen, Emmert, Scharer	Fusion Plasma Theory	276,000	365,000
Hershkowitz, et. al.	Experimental Investigations of Thermal Barriers	29,000	-0-
Wolfer	Effects of Radiation and High Heat Flux on the Performance of First Wall Components in Fusion Devices	-0-	66,000
Kulcinski, et. al.	Critical Issues for Magnetic Fusion Reactors	90,000	-0-
Kulcinski, et. al.	Fusion Reactor Physics and Technology Studies	450,000	350,000
J. Scharer	Wave Launcher Heating STudies in the Ion Cyclotron Frequency Range	-0-	38,000
J.C. Sprott, et. al.	Plasma Physics Research	915,000	525,000
		$3,397,000	$3,217,000

NSF SUPPORTED PLASMA PHYSICS RELATED RESEARCH

		FY 82	FY 83
J.L. Shohet	Microwave and Electromagnetic and Interactions with Plasmas Linear and Toroidal Geometry	$ 64,310	$122,622
J. Scharer	Ion Cyclotron Harmonic and Non-Electron Cyclotron Plasma Wave Studies	41,140	81,190
Emmert	Experimental Investigations of Plasma Instabilities	49,600	53,670
J. Tataronis	Low Frequency Wave Propagation in three-dimensional Geometry	38,000	41,000
N. Hershkowitz	Study of Non-Linear Wave Plasma Interactions	-0-	49,500
		$193,050	$347,982

TABLE II

Significant Achievements of the Tokapole/Multipole Program

1. Minimum-B stabilization of toroidal plasma.
2. Confinement clearly in excess of Bohm.
3. Influence of rotational transform on fluxtuations and transport.
4. Observation of convective cells and associated diffusion.
5. Hundred-fold increase in T_i with ICRH (to 600 eV).
6. Beta greater than (10x) MHD ballooning limit obtained.
7. Generation of poloidal divertor tokamak topology.
8. Comparison of stability of dee- and square-shaped plasmas.
9. Stable discharges with $q \approx 0.4$ obtained.
10. Demonstration of $\vec{E} \times \vec{B}$ divertor.

TABLE III

Significant Achievements Of The Phaedrus Tandem Mirror Program

1. Significant plasma heating in a tandem mirror end cell using RF (Radio Frequency waves) at the ion cyclotron frequency and at twice the ion cyclotron frequency.

2. First demonstration of plasma heating in a tandem mirror central cell using RF at the ion cyclotron frequency.

3. First experimental demonstration of MHD stability limits in a tandem mirror.

4. Production of tandem mirror configuration using RF alone, without the use of neutral beams (RF-sustained tandem mirror).

5. Density and energy buildup from combined use of RF and neutral beam injection.

6. Demonstration of electrostatic confinement of the central cell plasma in an RF-sustained tandem mirror by detailed mapping of the radial and axial structure of the electrostatic potentials in the device.

7. Production of an MHD stable axisymmetric mirror-confined plasma in the central cell without any end cells, using RF heating at the ion cyclotron frequency.

8. Demonstration of electrostatic potential modification by electron heating using RF at twice the electron cyclotron frequency.

9. Low power RF start up of an axisymmetric tandem mirror plasma.

TABLE IV

Significant Achievements of the Torsatron/Stellarator Laboratory

1. First 3-dimensional MHD equilibrium calculation for stellarators
2. First use of Alfvén waves for heating plasmas in stellarators
3. First calculation that showed that tangential neutral injection would heat plasmas in stellarators most successfully
4. First use of tangential plasma injection for stellarators
5. First design of a modular stellarator
6. First construction of a modular stellarator (design won the 1982 award of Design News as the most innovative engineering design)
7. First fully three dimensional ballooning mode stability calculation
8. First analysis of particle transport in stellarators that reconciled numerical and analytic work. It showed that the transport can be much better than previously thought.
9. First direct measurement of electron heat transport in stellarators
10. First direct measurement of the bootstrap and Pfirsch-Schluter currents
11. First proof that super bananas do not exist in certain types of stellarators

TABLE V

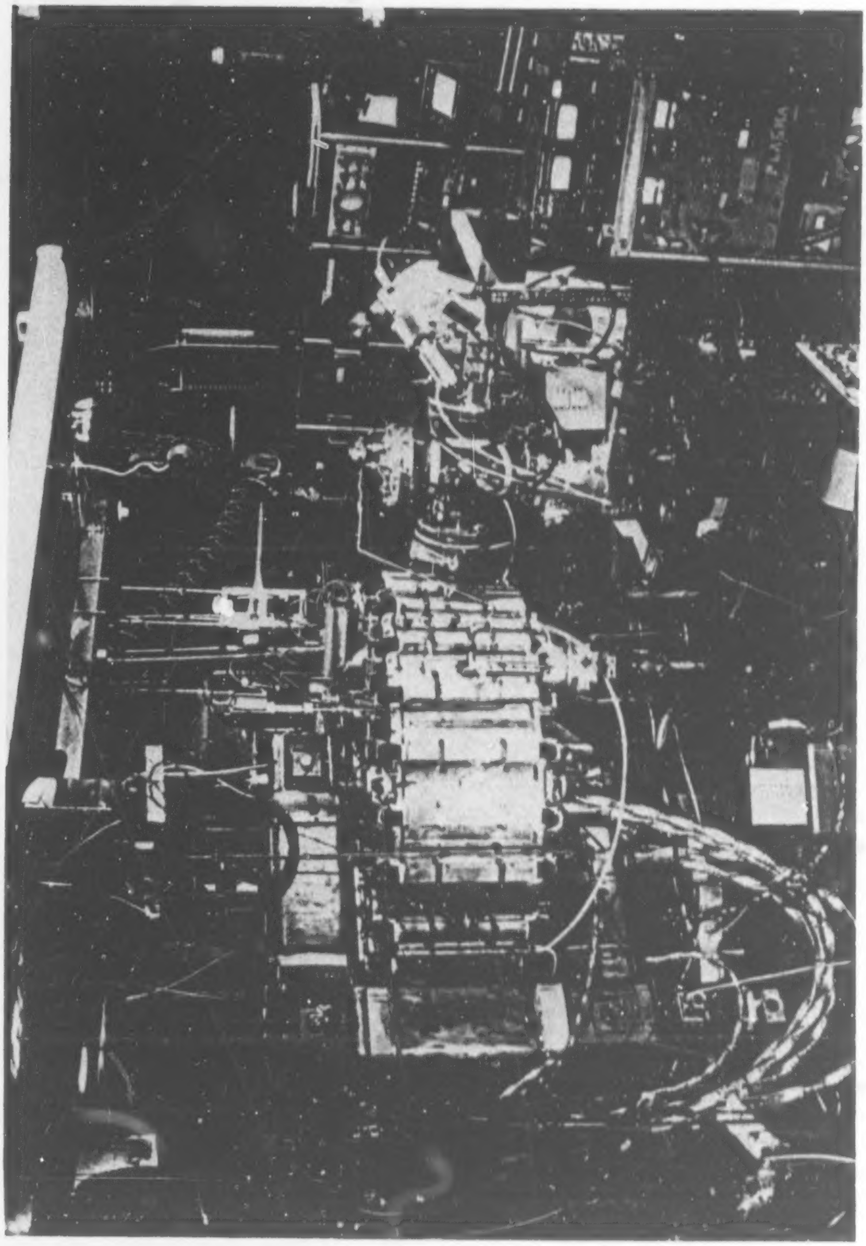
Significant Achievements of the Fusion Engineering Program

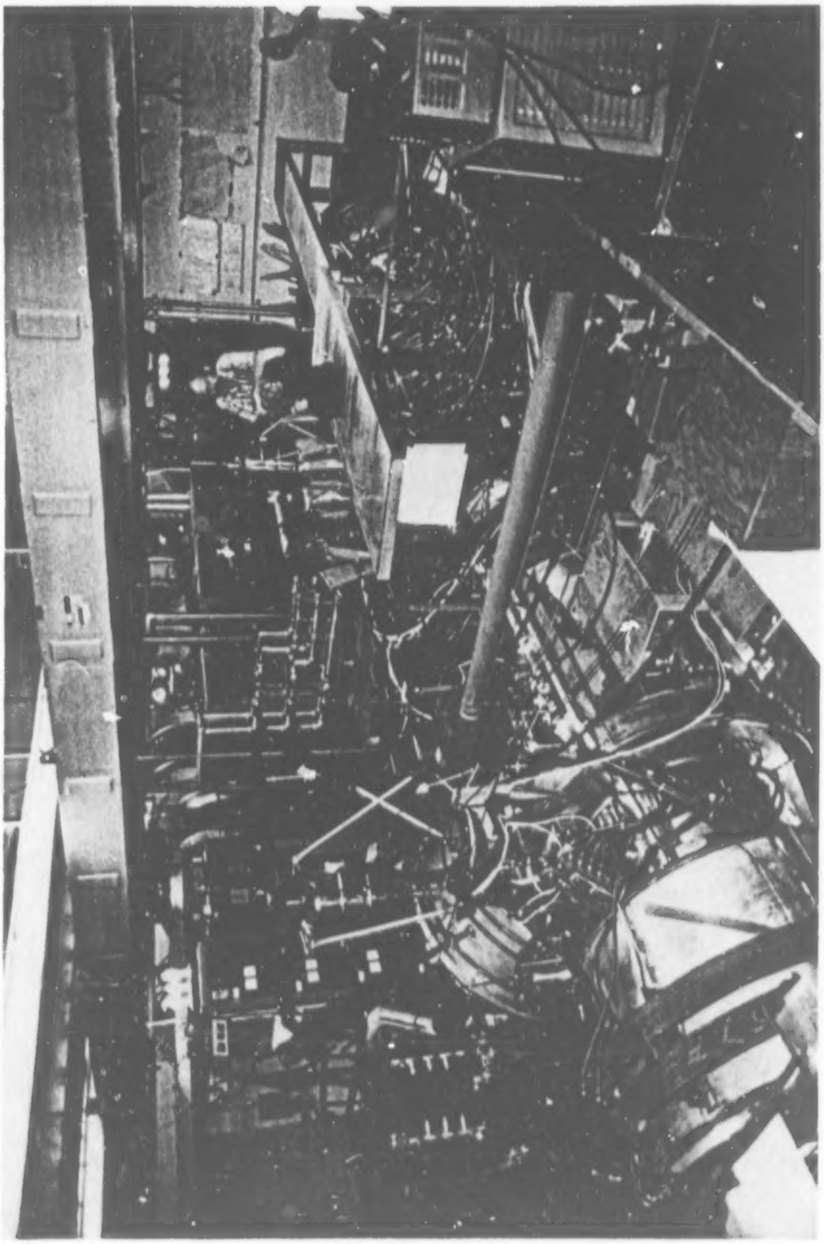
1. Double Null Poloidal Divertor.
2. Carbon Curtain Protection from disruptions.
3. Imbedded S/C Magnet Conductor Design.
4. Thermal Flywheel for Tokamaks.
5. Sloshing Ions in Central Cell of Tandem Mirror.
6. RF heated commercial Tokamak Reactor.
7. First Modular Stellarator.
8. Nb-Ta-Ti S/C Magnets.
9. H_2O Coolant of Fusion Blanket.
10. 3-D Monte Carlo Neutronics of Blanket and Penetrations.

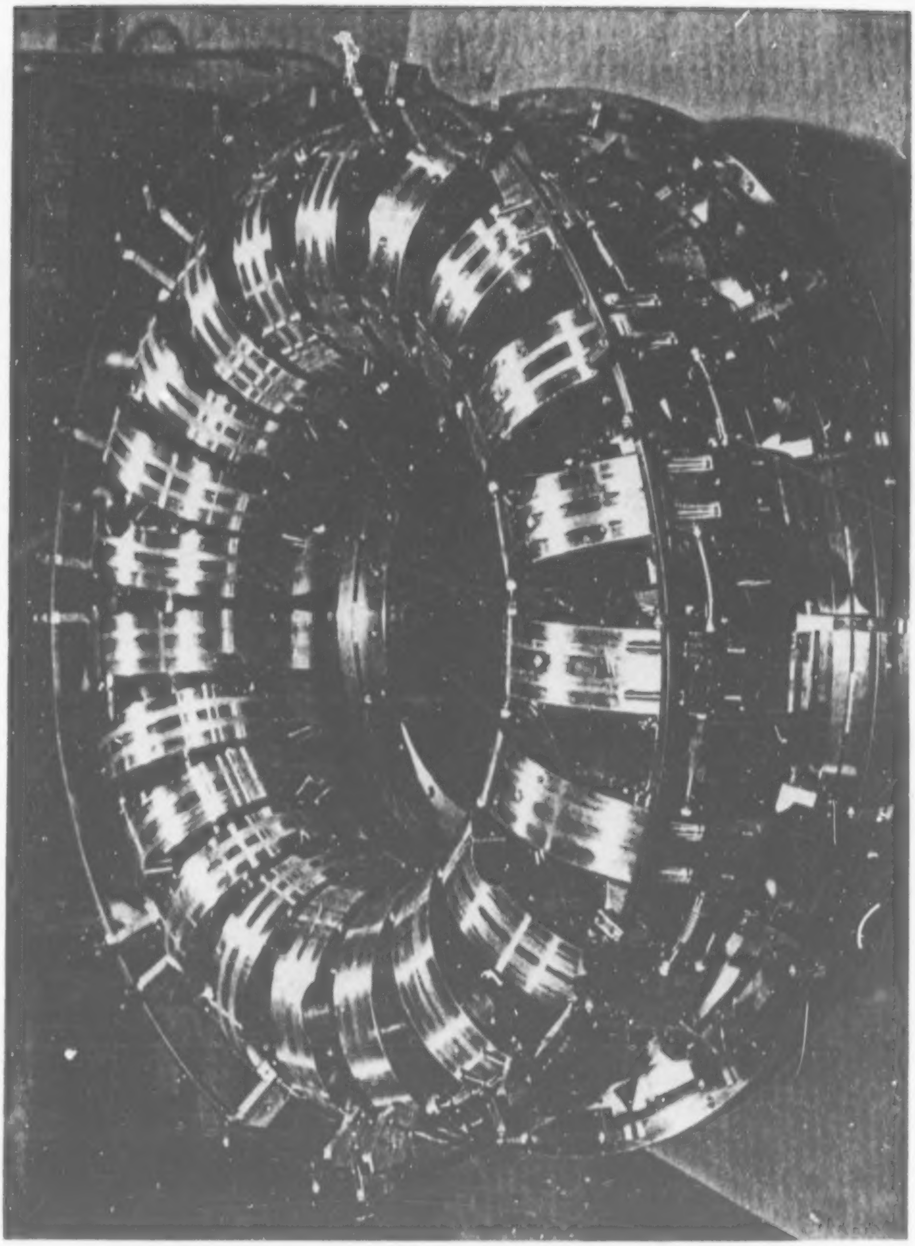
TABLE VI

Significant Achievements of the Fusion Plasma Theory Program

1. Analytic calculations of barrier-trapping currents in tandem mirrors with thermal barriers.
2. Improved calculations of neo-classical transport in rippled tokamaks and stellarators.
3. Calculation of plasma flows and the bootstrap current in non-axisymmetric toroidal devices such as stellarators.
4. Investigation of rippling instabilities as a mechanism causing tokamak edge plasma turbulence.
5. Evaluation of neo-classical currents including bootstrap current in conjunction with experimental observations in the large octupole.
6. Estimation of ionization effects on self-consistent potentials in divertors.
7. Development of ray tracing codes for ECRH heating in tandem mirrors.
8. Coupling of ECH ray tracing code to transport simulation code for tandem mirrors.
9. Development of models for ICRF coupling and heating in tokamaks.







UNIVERSITY FUSION ASSOCIATION

OFFICE OF THE CHAIRMAN
of the Executive Committee
J. L. Shohet

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Boston College
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STATEMENT OF

DR. J. LEON SHOHET, CHAIRMAN

UNIVERSITY FUSION ASSOCIATION

BEFORE THE

SUBCOMMITTEE ON

ENERGY RESEARCH AND PRODUCTION

HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

ON

THE DEPARTMENT OF ENERGY FY 1984 BUDGET

MARCH 16, 1983

SUMMARY

The University Fusion Association agrees with the basic philosophy expressed in the DOE Budget Request -- support of high-risk, long-term, high-payoff programs. We urge the subcommittee to consider the following points regarding the role of universities in fusion:

1. Universities should be considered a national resource for research; industry a national resource for product development.

2. The present fusion budget provides only slightly more than 4% of its total to universities and has not kept pace with the scope and needs of the fusion program. This is an unconscionable loss of the great resources available in universities today. The High Energy Physics Program of DOE is an example of an appropriate relationship between universities and national laboratories.

3. Universities have contributed key ideas to the fusion program, but will no longer be able to do so unless they can be assured of continued, long term funding and are not forced into a role of simply providing service to the national labs. Otherwise, key personnel will be lost, competent research teams will be broken up, and university administrations will no longer encourage faculty to become involved in fusion.

4. Joint university-industry programs are both productive and very cost effective.

5. Universities are the only source of newly trained manpower for fusion and can only continue to attract and train "the best and the brightest" if the scope of the programs are meaningful.

6. All possible participants should be able to compete fairly and to propose and develop their ideas without the built-in biases that currently exist in OFE. They should receive a fair hearing and funding if the research is deemed relevant and if they are judged competent to perform the research.

I am Leon Shohet, and I am a Professor of Electrical & Computer Engineering at the University of Wisconsin, and I am also the Director of the Torsatron/Stellarator Laboratory, a magnetic fusion research facility at the University. I am pleased to speak to you today on behalf of the faculty and research staff at 50 of our nation's colleges and universities, all actively engaged in fusion research. As Chairman of the Executive Committee of the University Fusion Association, it is my pleasure to have the opportunity to appear and make a brief statement on behalf of the UFA before the Subcommittee on Energy Research and Production of the House Committee on Science and Technology.

We strongly agree with the philosophy expressed in the Reagan budget for the Department of Energy, namely that DOE should support those items that are long term, high risk and with high potential payoff. By high risk we mean in an economic, not a safety sense, and by high payoff we mean those projects that offer a substantial advantage in economics, safety and environmental factors. We believe that fusion meets these requirements. We appreciate your efforts on behalf of the fusion community of the nation's universities, because we know that the task of authorizing the fusion budget is a very difficult one.

We believe that it is important for you to gain some perspective on the university role in the fusion program. From this I think, you will see just what we perceive are the problems and opportunities for universities in fusion.

As you well know, the budget for fusion has grown considerably from its early years. Substantial research establishments exist at several of our national laboratories. It is important to note that a similar growth in university activity in fusion has not taken place. For FY 1984, DOE has allocated only slightly more than 4 percent ($21 million) for university research, out of approximately $450 million. We think that this has happened because of the shifting emphasis from research to relying on a few options in the program along with rigid direction from DOE/Washington. As a result, the continued development of basic fusion science and its codification into the body of scientific information to be passed on to the next generation of researchers has been continually downgraded in favor of accelerated progress in the major projects. That is, the ever-increasing programmatics has caused a number of university programs to be squeezed out or have their budgets substantially cut and new proposals from universities are often not funded. It is folly to terminate a $50-100K/year university program to buy another pump for a beam line on a major machine.

While it may be true that funding goes from national laboratories to industry and some to universities, there are many problems with this arrangement. First, the national laboratories charge a fee for passing through the funds, often as much as 20%. Secondly, when budgets get tight, as they often do, the outside contractors and/or universities are the first to be affected, as I am sure you can well understand. The third problem is that the prime contractor is the national laboratory and not the university or industry. Thus, it appears to be current DOE policy that both universities and industry act only to service the needs of the national laboratories.

This, in our opinion, is wrong. The goal of fusion research is to make fusion, not to support a group of national laboratories. Universities must be considered as a national resource for research, and industry must be considered as a national resource for product development. National laboratories, on the other hand, should do those projects for which universities and/or industry are not well equipped and should not unilaterally expand their missions to be all encompassing.

University activities broaden the base of the fusion program. They provide new ideas and independent evaluation. In the experimental areas, universities are significantly more cost effective and more flexible than the national laboratories and industry and have provided outstanding new ideas which have often been taken over by the national laboratories. In fact, we see one of the major problems with the fusion program today is funding for universities is often designed so that new ideas cannot be fully developed at universities. One often gets the impression that not only do national laboratories want to do the major programmatic experiments, they want to do all the experiments. There are several examples of these experiments currently in operation at national laboratories which are of the scale and mission of those normally thought to be done at universities.

Nevertheless, we do support the idea of effective university-national laboratory cooperation. However, my frank observation, covering a period of over 20 years, is that there has been very little meaningful interaction. It is certainly true that faculty members have spent research leave at national laboratories, students have completed their theses at these institutions, and some diagnostic development and measurements have been allowed. Yet, nowhere in the fusion program have I seen a joint national laboratory-university design of a major experiment. Having students and staff on the national laboratory site is crippling to the university's research program, removing, as it does, the staff member, the graduate student, and the research itself from the university environment. National laboratory staff members do not seem to appreciate that a principal reason for doing research on campus is to establish and maintain a research environment into which new students can continually feed.

Another reason for the problem is that national laboratory staff members generally feel that they do not need university participation. The outstanding exception to this has been in the case of high energy physics Here, where it is obvious to everyone that the outstanding researchers are at universities, the labs have needed the universities and vice versa, and the cooperation has been extensive and fruitful. High Energy Physics utilizes the recommendations of an advisory board, in which university representation predominates. As a result, the entire community, including scientists, engineers and university administration, supports and trusts the DOE administration of the program, because it clearly follows the recommendations of the advisory board. A similar arrangement for fusion would most certainly bring about a good image and attract bright newcomers to the field.

In fusion, the national laboratories have not perceived a need nor developed an effective mechanism for using the intellectual resources available at universities. I think this results in part, from feelings of the national laboratories and the DOE Office of Fusion Energy that university staff members and students cannot get things done on schedule. Another argument frequently used by DOE and those who argue in favor of having the research done at a national laboratory is that many of the programs are large and multidisciplinary and thus unsuited for a university. These arguments have weight. However, more than one example can be cited of large, multidisciplinary federally funded fusion research programs carried on successfully by universities when funding has been provided on a firm basis for some reasonable period of time. In the U.S. fusion program, the TEXT tokamak ($10 million construction cost) at the University of Texas, the Phaedrus experiment at the University of Wisconsin ($10 million) and the TARA experiment at MIT ($18 million) are superb examples of the level and type of program that has been successfully managed at universities in fusion. In Japan, the $35 million Heliotron-E experiment, built jointly with industry, is now operated by a staff of under a dozen people that includes several graduate students at Kyoto University with a Professor of Electrical Engineering in charge. All of these programs are very small compared to national laboratory activities, but all have contributed far out of proportion to their cost.

UFA is confident that strong, productive and cooperative research programs between the national laboratories, universities and industry can be done. University overhead rates are significantly lower than those of national laboratories, they do not have nor require large in-house construction and design groups, since much of what is done can be designed in cooperation with industry. It is also most important to note that this research makes a significant contribution towards the training of the next generation of scientists and engineers. This constant flow of new personnel, largely in the form of graduate students and post-doctoral people, results in a stimulating environment for the development of new ideas and concepts. Examples of this include: the rebirth of stellarators in the U.S., the importance of beta for fusion reactors, 3-dimensional computer codes and numerical simulation, high field tokamak operation, r.f. current drive, discharge cleaning, and major theoretical contributions to tokamaks and tandem mirrors.

We must provide a mechanism for all possible participants, either at national laboratories, industry or universities to develop and propose the most meaningful, cost-effective and relevant ideas, rather than relegating industry and universities to a subservient role to the labs. If the reviews are good and an institution is judged capable of carrying out the research, it should be funded. Nobody has a monopoly on good ideas and we need the best of all of them from all sectors to solve the fusion problem.

As an example of the problems with funding that universities face, I would like to relate the following situation to you. We were pleased to note that in the FY 1983 continuing resolution for DOE, Congress saw fit to appropriate funds for the design of a next-generation experiment at a university. Unfortunately, the DOE management has seen fit to oppose the will of the Congress and has steadfastly refused to let the contract be funded that Congress directed.

In closing, I must point out that this testimony represents a summary of the opinions of several members of our executive committee. I was impressed by the fact that the general tone of all of the comments has been very similar, universities have not been able to contribute to the fusion program because of the lack of adequate vehicles and financial resources. It is our hope that you will assist us in contributing toward the goal we all seek--commercial fusion power.

DR. J. LEON SHOHET

Dr. J. Leon Shohet is Professor of Electrical and Computer Engineering at the University of Wisconsin-Madison. He received his B.S. degree in electrical engineering from Purdue University in 1958 and the M.S. and Ph.D. degrees in electrical engineering from Carnegie-Mellon University in 1960 and 1961, respectively.

Honors include: Frederick Emmons Terman Award of the American Society for Engineering Education; Merit Award of the Nuclear and Plasma Sciences Society of the Institute of Electrical and Electronics Engineers; Fellow of the American Physical Society; Fellow of the Institute of Electrical and Electronics Engineers; Distinguished Student at Purdue University; Membership in Eta Kappa Nu and Tau Beta Pi.

Dr. Shohet is the author of a major textbook in the field of plasma and fusion science and has written a chapter on stellarators for a book on fusion edited by Edward Teller. He is the Chairman of the Coordinating Committee on Plasmas at the University of Wisconsin and director of the University's torsatron/stellarator laboratory. In addition, he is the author of over 200 publications on plasma physics, magnetic and inertial fusion, and biophysics.

He is a member of the U.S. Department of Energy stellarator advisory committee and also a member of the DOE-EURATOM steering committee on stellarators. He is a member of several professional organizations, including the Institute of Electrical and Electronics Engineers (in which he has served as president of the Nuclear and Plasma Sciences Society and a member of the IEEE Technical Activities Board and is currently the chairman of the IEEE Research and Development Committee), the American Physical Society and the American Association for the Advancement of Science. In 1973 Dr. Shohet founded the Institute for Electrical and Electronics publication Transactions on Plasma Science, which was the first engineering publication devoted to the area of plasma physics and fusion. Dr. Shohet has served as consultant to numerous U.S. national laboratories and industries. He is currently Chairman of the University Fusion Association.

He is listed in Who's Who in the Midwest and in American Men and Women of Science.

Mr. Hall. Dr. Shohet, thank you very much.

We will reserve the questions until we have heard from Mr. Dawson and Dr. Drummond.

The Chair recognizes Mr. Dawson.

STATEMENT OF JOHN M. DAWSON, DIRECTOR, CENTER FOR PLASMA PHYSICS AND FUSION ENGINEERING, UNIVERSITY OF CALIFORNIA AT LOS ANGELES

Mr. Dawson. Mr. Chairman, it is a pleasure to be here today.

I am a professor in the physics department at UCLA and director of the Center for Plasma Physics and Fusion Engineering at UCLA.

The program at UCLA consists of essentially four parts.

First, there is a part on fusion reactor engineering, under Bob Conn. There is a program on fusion experiments. This consists of two parts. The first part is involved with tokamaks. These are the microtor and macrotor. That program is directed by Bob Taylor.

The second part of the program deals with mirrors. The lamax is the primary machine there. That program is directed by Al Wong.

Up until about a year ago there was also a multipole experiment, but that was terminated. That was also under Al Wong.

There is also a group doing basic plasma experiments. This is primarily under Professors Stenzel and Wong.

I might also include under basic experiments diagnostic development research under Professor Luhmann.

The other two parts of the fusion program are an effort on plasma theory under Professor Fried and a program on computer modeling under myself.

These programs are distributed between the physics, astronomy departments and the electrical, and nuclear engineering groups and a few other departments.

Overall there are 16 faculty members, 87 research and support staff, and 60 graduate students at UCLA. The overall budget for the plasma program is $7.7 million, of which slightly less than two-thirds comes from DOE for fusion research.

There is an expanded version of the handout, of which I only brought about 25 copies. I do not know whether you have that, but attached to the testimony is a description of the UCLA program.

Mr. Hall. Yes, we have that.

Mr. Dawson. Now I will give a somewhat abbreviated version of the first six pages of the testimony here.

First of all, I might start out by describing what I consider the university's role in the fusion program to be.

The universities represent a resource for the fusion effort, and it has many facets. First of all, and I suppose primarily, is the training of scientists and engineers who will ultimately take over and lead the program. Universities are a source of relatively unbiased, trained people for reviewing and evaluating DOE programs and giving advice on committees and things such as that.

They provide contact with industry through consulting of the faculty and through their students who go to work in industry.

They provide contact with developments in other areas of science and technology which may be beneficial to the fusion programs.

These areas of science and technology exist at universities and professors have contacts with them.

They provide additional resources for the fusion program through the faculty salaries, which are generally supported by the university; through laboratory and office space; and student support, et cetera.

They provide a window through which the general public can see and become informed about the fusion program.

They provide a window whereby technological developments in fusion research can become known to other branches of science and technology and also to industry, so that these developments can be incorporated in the general scientific and industrial programs of the United States.

The present DOE policy does tap these resources. It taps some of them to a larger extent than others. However, I think that it falls far short of tapping these resources to the optimum extent.

First of all, there is no systematic program to deal with the training of young scientists. Many of the scientists in the fusion field were trained in other fields and have transferred to the fusion program. It takes a long time to train a scientist. It takes 4 or 5 years from B.S. to Ph. D.

Continuity of support is extremely important. Modern equipment, access to fusion-type plasmas are essential. Exciting university programs are essential to attract good students and good professors to the program.

These programs need some stability to succeed in attracting these good people.

University programs, by and large, are small efforts, and DOE cannot give them the individual attention that it does to the national laboratories. DOE tends to concentrate its attention on the national labs, which are, of course, the cutting edge of the fusion research, but the university programs form the roots of the program. If you do not tend the roots, the plant will die.

Many innovative ideas that could be of great value to the program come out of the universities. Often, good ideas are not funded because they are not of immediate applicability to the major devices at the national labs.

Some relatively small university programs, which I would call high-leverage items, are not funded. By high-leverage items, I mean that if they are successful, these programs could lead to great savings in terms of the fusion program. For example, heating equipment might be greatly simplified and the costs reduced by the development of negative ion beams or other forms of heating, of which there are abundant numbers of ideas in universities but most of these programs do not get funded.

Generally, relatively modest funding is required to carry on this research, but still the universities find it very difficult to get it funded.

With regard to cooperation between universities and industry, there is not much done and there is not much encouragement for it. I think successful ideas developed at universities might be—successful ideas which have been proven at universities might be—developed by industry by some appropriate arrangement. Some of this might be done by simply assuring industry that the fusion program would buy the product if it met some need of the program. In this way, some industrial money might be brought into the program. It might also ensure or bring about the funding of university programs by industry, which would also be helpful.

In your letter you asked what responsibilities and mechanisms are appropriate for the Department in professional level manpower development, including but not limited to support of graduate students, postdoctoral researchers, and young faculty members. In this regard, you might consider the following:

Presently, at most universities faculty positions are very tight. It is very difficult to obtain faculty positions for young scientists. Funding of, say, 5-year careerships or things like this for the brightest of the young people could give them the start they need and, in the end, they could become the professors at universities or go into the national labs or perhaps into industry and become the leaders of the program.

Funding of exciting research programs at universities with fusion-type plasmas and up-to-date diagnostics I think are very important. Without these programs, we will not attract the good people we need. Such programs should have some stability of funding, perhaps some kind of multiyear commitment. This might be given contingent upon performance and, of course, funds available. Similar forms of funding are done by NSF, and I think that it works rather well there.

Stability might also be provided by something which I would call different colors of money. This refers to one of the problems Leon mentioned. Moneys for the universities should be insulated and not be tappable by the large national lab programs. If this is not done and a national lab program hiccups or runs into trouble, if they drop a generator or something like that, it is easy to wipe out the whole university program just to try to bring the national lab back on its time scale. I think it is much more preferable to let the lab program slip by a little bit than to wipe out the university programs.

One suggestion I have is that perhaps there should be a special office of DOE devoted to considering university problems and programs and the contributions they make. Perhaps such an office, in consultation with the universities, could determine what the appropriate DOE policy toward the university programs should be. Such an office might try to foster interaction between universities and national labs and vice versa. It could also bring to the attention of the national labs developments at the universities which tend to be overlooked.

Also in your letter you asked what current factors, actual or perceived, foster or impede good working relations between the Department and the academic community. Well, firs of all, some of the things that foster good working relations are:

There is relatively strong financial support for universities through DOE, although it is somewhat erratic, as we have already heard. The strong national programs at the labs, of course, are very attractive for researchers to interact with. The availability of such facilities as the Magnetic Fusion Energy Computer Center and the Texas Tokamak, the Institute for Fusion Studies, things like that, are all very beneficial.

Now some factors which are actual or perceived which impede good working relations:

First of all, there is a large amount of paperwork required for relatively small projects with no assurance that one will be successful even if the idea is good and receives good reviews. This is very discouraging and turns off a large number of faculty members from getting involved.

Rightly or wrongly, it is perceived that many of DOE's decisions are at least strongly influenced by political considerations. If a university is in competition with a national lab, the program almost invariably goes to the national lab. Now this may be appropriate because the lab has better facilities, more up-to-date equipment, things like that, but if this is the case, I think at least some kind of joint program between the university and the lab should be initiated, if the idea originated in the university, so that the universities feel that they are playing an important role in the program.

In your letter you also ask, in what way, if any, can the Department improve the effectiveness of the working relation between the Department of Energy laboratories and the universities? Well, the joint research programs which I just mentioned are, of course, one possibility.

There are also cases where some encouragement of programs at the universities by the laboratories, perhaps on small things, on the small high-leverage items, which I mentioned, which if they turned out to be successful could impact the national lab programs 5 years down the road, would be very helpful, I think. Particularly things along the lines of innovative schemes for heating fusion devices, new ideas and concepts for confinement, small-scale experiments along those lines would be very helpful, if there were encouragement from the labs and even participation in the labs by sending people to help out with some of these things.

You also asked in your letter what changes, if any, should the Department make in order to provide more effective support of the university-based research. I think the primary need is a consistent policy toward the universities and what they do. Stable research funding is needed to attract good students and good faculty members. Exciting programs of research with fusion-type plasmas and modern diagnostics and equipment at the universities are necessary.

Better means of interfacing between universities and national labs are needed. If there were some provision so that the good students from the universities could go and work on some of the exciting experiments at the national labs, that would also be beneficial.

That is the end of my testimony.

[The prepared statement of Mr. Dawson follows:]

PREPARED STATEMENT OF JOHN M. DAWSON

Let me first address the questions of Is the Department of Energy effectively utilizing the general capabilities of the university community in both basic research and engineering development? Is the Department of Energy effectively utilizing universities vis a vie laboratories and industry and basic research in engineering development? The universities represent a resource for the fusion effort with many facets: the training of scientists and engineers who will ultimately come to lead the program, they are a source of new ideas and understanding, they are a source of relatively unbiased trained people for reviewing and evaluating DOE programs, they provide contact with industry through the consulting of faculty and through their students that go to work for industry, they provide contact with developments in other areas of science and technology which may be beneficial to the fusion program, they provide additional resources to the fusion program through faculty salaries, laboratory and office space, student support, etc., they provide a window through which the general public can see and become informed about the fusion programs, they provide a window whereby technological developments in fusion research become known to other branches of science and technology and industry so that these developments can be incorporated in the general scientific and industrial programs of the United States. The present DOE policy does tap into these national resources, some aspects are made better use of than others. I think the program gains a great deal of benefit from this interaction. However, I believe that the Department of Energy is far from making optimum use of this resource. First there is essentially no policy for training of students or post-docs; it is left to a rather haphazard process. Many of the people working in fusion have been recruited from other areas. The training of students takes a long time--4 or 5 years from the BS to the Ph.D. Continuity of support, modern equipment, access to fusion-type plasmas, are essential, exciting university programs are needed which will attract both good faculty and students. These programs must have some stability to succeed at this. Also, if a faculty member or his students have good ideas which can be important to the fusion program, he needs to know that there is a good chance that his program will get funded in a reasonable length of time. Present DOE funding does much to provide for these needs but fall far short of being a definite policy.

University programs by and large are small efforts and DOE cannot given them individually a lot of attention. DOE tends to concentrate its attention on the efforts at national labs, which are of course, at the cutting edge of fusion research. This has both advantages and disadvantages. The advantages are that universities have somewhat more freedom, although DOE still has a tendency to try and run the university programs the same way that it runs the national labs programs--setting rigid milestones and reporting requirements that don't fit well with programs of training students and small research efforts, many of which are based on new ideas which are quite exploratory in nature. The disadvantages are that much of the good university research gets overlooked or ignored, both by DOE and the national labs. Many innovative ideas that could be of great value to the program are never funded because they are not of immediate applicability to one of the major devices. Some relatively small university programs are programs which I would call high-leverage items. That is, if they are successful they could save the fusion program much money and make qualitative differences in the overall program, and possibly even the design of final reactors. Generally relatively modest funding is required for this kind of research but still it is often very difficult to get it funded because some risk is involved and it cannot be applied to the main program perhaps for five or ten years. However, in a long-range program like the fusion program is, such research can be of immense value, as has been quite well born out by a large number of unexpected but highly favorable and important discoveries. Perhaps the best example of this is the confinement scaling discovered by research at M.I.T. on the Alcator projects.

Not much (essentially none) attention is given to links between universities and industry. Successful ideas developed at universities could be developed by industry by appropriate arrangements. Some of this might be done simply by assuring industry that the development of equipment, if it meets the needs of the fusion program, would be bought by the fusion program, in this way additional industrial funding might be brought into the program or such funding might be provided to universities to follow up on some of their ideas.

At present, with few exceptions, the national labs are wrapped up in their own programs and pay relatively little attention to or give much encouragement to university programs. They are mainly interested in efforts which will push what they are doing at the present time forward and show little interest in innovations which might impact their programs five or more years down the road. This attitude is understandable but for a long range program like the fusion program, such an extremely conservative approach is not beneficial.

Question: what responsibilities and mechanisms are appropriate for the Department in professional level manpower development including but not limited to support of graduate students, post doctoral researchers, and young faculty members? In this regard, the following might be considered: present faculty positions are very tight at most universities. Funding of say five year career ships for talented young people at universities can be an invaluable help to get them started. These people might later become faculty members or go on to work at national laboratories or in industrial programs involved in the fusion program. Funding of exciting research programs at universities with fusion type plasmas and up-to-date diagnostics are very important. Such programs should have some stable multiple year support contingent upon performance and funds available; something along the lines of what is done by NSF. Stability might be provided by having what I call different colors of money. Monies for the university programs should be insulated from being tapped by the big national lab programs so that if one of these programs hiccoughs, there is no danger that it will wipe out a large segment of the university effort. I believe an approach similar to this is used by the DOD. Programs for the best students to work at national labs, particularly on cooperative programs between the labs and universities would be invaluable. However, such arrangements cannot take the place of having good exciting programs at the universities. Some arrangements for members of the national labs to spend time at universities lecturing or teaching and interacting with students would also be a valuable aid for training and attracting good students. A special office of DOE devoted to considering the university programs and their unique problems and contributions might be of help. Perhaps it could, in consultation with the universities, determine what the appropriate DOE policy towards university programs should be. Such an office might try to foster interactions between universities and the national labs and vice versa. It could also bring to the attention of the labs developments at the universities.

Question: What current factors, actual or perceived, foster or impede good working relations between the Department and the academic community? First, those factors which foster the relation. The relatively strong monetary support given the universities allows them to have programs which can interact effectively with the national labs. Strong programs at the national labs do attract many university researchers to go and visit them and spend time there. The availability of facilities such as the MFE Computer Center, the Texas Tokamak, the Institute for Fusion Studies, all aid the university programs. Factors actual or perceived which impede good working relationships. The large amount of paper work and red tape required for relatively small projects is a serious burden for universities. The length of time it takes to get a new project or innovation funded discourages many researchers from going after support or discussing their ideas with the Department of Energy. The fact that many ideas which receive good reviews and are highly recommended don't ever get funded discourages researchers and is very bad for moral. It is perceived that many of the DOE decisions are at least strongly influenced by political considerations. If a university is a competitor with a national lab, the program almost invariably goes to the national lab. In many cases, the innovator of the program may be at the university but in the end, he finds that he has been pushed out of the research effort. Some of these projects are of course better done at the national labs where the facilities are better; however, if in these cases some form of joint program with the universities could be established so that they feel they have a part and a stake in bringing forward their ideas, I think that would be a great improvement.

Question: In what way, if any, can the Department improve the effectiveness of the working relationships between the Department of Energy laboratories and the universities? Joint research programs between laboratories and universities would be a step in this direction, particularly where ideas which originate in universities are involved. Projects too large or requiring greater resources than the universities have might go to the national labs, but it would be very valuable if the originator of such an idea could continue to be involved in it. There are also cases where joint programs could be undertaken where it would be appropriate for the research to be done at the university. Such programs might make use of special expertise that exists at universities, they might involve small programs which the laboratories do not want to devote the manpower or resources to, but which have the potential to pay off for the laboratories on a longer time scale. Better methods of making the laboratories aware of developments at universities are needed, enhanced exchange mechanisms or personnel between universities and laboratories could be helpful in this regard. I believe that also joint efforts involving universities, national labs and industry could also be very valuable.

Question: What changes if any, should the Department in order to provide more effective support of the university based research? The primary need is for a consistent policy towards universities and what they do. Stable research funding is needed to attract good students and good faculty members. Exciting programs of research with fusion type plasmas, modern diagnostics, and other equipment are essential to the maintenance of viable programs at the universities. Better means for interfacing the universities with the national labs so that our best students might go and work on the most exciting experiments would also be a very useful development.

Outline of UCLA's Plasma and Fusion Program

The UCLA fusion and plasma program has branches in the Physics and the Astronomy Departments of the College of Letters and Science, in the Electrical Engineering and Nuclear Engineering groups of the School of Engineering and Applied Sciences and in the Institute of Geophysics and Planetary Physics. All these efforts are grouped together in the Center for Plasma Physics and Fusion Engineering and these efforts are loosely coordinated by the Center. The UCLA effort consists of some 16 faculty members, 87 research and support staff and 60 graduate students. The overall plasma and fusion budget is $7.7 million dollars per year, of which a little less than 2/3 comes from DOE for fusion research.

Attached is a brief description of the UCLA program taken from the Project Planning Guide for the Plasma/Fusion Energy Programs; a document prepared in 1982 as a first step in obtaining a building to house the major part of the Plasma Fusion efforts.

Mr. HALL. Mr. Dawson, thank you very much.

Dr. Drummond.

STATEMENT OF DR. WILLIAM E. DRUMMOND, DIRECTOR, FUSION RESEARCH CENTER, UNIVERSITY OF TEXAS AT AUSTIN

Dr. DRUMMOND. Mr. Chairman, I appreciate the opportunity to be here to discuss the university fusion programs, and in particular the fusion program at the University of Texas.

This marks my 25th year in fusion research. I am struck by the difference and the dramatic progress that has been made. In looking back over these 25 years, it is almost impossible to have imagined what we see today in the overall fusion program compared to what we saw 25 years ago.

Fusion has been called the most difficult scientific challenge of the 20th century, as well as the key to an environmentally attractive and economically strong 21st century.

I think you will get a very good feeling from the testimony, particularly in the next 2 days or so, that the DOE fusion program is, indeed, a carefully thoughtout program which is making steady progress toward that distant but highly visible goal. It is somewhat like a train that is going up a long incline. It started slowly and it is a long incline, but the train is now moving fairly steadily. I think the fusion program has made major progress, both scientifically and in terms of management.

In particultar, if we ask what is the key to the progress that we made, it is the steadily growing understanding of the basic mechanisms of fusion plasmas. I think our understanding of how plasmas behaved 25 years ago was simply incredibly small, and today is has grown a great deal. It is that understanding of the basic mechanisms of plasmas which we have gotten from both large laboratory and small university experiments, from trying to interpret small experiments to see if theories work, and, in good measure, from the strong theory program which has always been a part of the U.S. fusion program.

I think the thing that is driving this train up the hill is our basic scientific understanding of fusion plasmas. That basic understanding is the engine, and you cannot get there without the engine.

The crucial thing that universities have been able to do, and should be able to do in the future, is contribute to that basic knowledge of fusion plasmas. That is the most important thing. I am speaking as a scientist as distinguished from an engineer.

We have not come as far as we need to come in the understanding of fusion plasmas to make a commercial fusion reactor from the scientific point of view. We simply do not know that much. We have come a long way, though, and it is important that we continue that growth in our understanding of plasmas. That the key role that universities can and do provide.

The University of Texas is particularly blessed, I think, with programs which in some measure meet the objections or the difficulties that Leon Shohet spoke to earlier and that John Dawson mentioned. We have three types of programs at Texas.

First, let me speak of the TEXT tokamak device. This is a large modern tokamak facility of high quality that was just completed and chartered by DOE to explore a wide variety of fusion phenomena. It was not dedicated to a particular line of experiments. It is open and available to all qualified researchers from other universities and from the national laboratories. Indeed, John knows the people from UCLA are working in our laboratory. They and scientists from GA are currently doing experiments on TEXT.

It provides an opportunity for an expensive device to be shared by many people. This provides one which is not in the national laboratory, not preprogramed, and which is available to be used flexibly.

One of the things that has been a difficulty in the past is that it has been hard for the university researcher to try out his ideas in a national laboratory major experiment. That is not hard to understand. When somebody constructs a $100 million experiment, he has a program plan for many years in advance. Not only programmatically, but from an engineering point of view, he simply cannot open up his machine and put somebody else's diagnostics in it very often. Doublet III, I think is designed to be opened only once or twice a year for the introduction of new diagnostics, whereas the tokamak at Texas is designed to be opened for this purpose every 2 weeks.

It provides a users' facility for people both in the national laboratories and in universities to come and not only try out diagnostics, but try out some of those new and novel ideas that cannot be tried in the national laboratories simply because it is programmatically impossible to interrupt the tightly controlled schedules of the large experiments.

The TEXT facility is something that makes it very easy for us to feel that we are making a contribution.

I should say that the lack of facilities available at national laboratories is a problem that is shared not only by university people, but by people at the national labs. Half of our customers, so to speak, of our users of the TEXT facility, come from national laboratories where they cannot get time on the machines at the national labs. That is one way in which that kind of problem is being met.

The second major thing that is a DOE facility almost exclusively is the Institute for Fusion Studies, which after a nationwide competition was awarded by the DOE to the University of Texas. It is a national center for fusion theory. It was not designed to replace fusion theory groups at the national labs or at universities. It was not to be a reshuffling of existing groups. It was to supplement in a very specific way the efforts in the major laboratories.

If you look at a major laboratory and ask what do their theory group work on, you will find they primarily work on those kinds of scientific problems that are relevant to the large experiments in those laboratories.

The charter of the Institute for Fusion Studies was to look across the broad sweep of problems and devices in fusion and try to see the similarities, the differences; to try to develop the basic understanding of plasmas that is common to all of the different devices. As a result, we find there is a lot of synergism.

Recently, some work that we were doing for mirror machines at Livermore turned out to be quite applicable to the Elmo Bumpy Torus program. It was extended to help that program, to help our understanding of it. Then, within the past 2 weeks, it was again further extended to provide at least a new theoretical approach to removing the power density limitations in tokamaks. All of that came together because we had a group which was large enough and strong enough to look across the whole swim.

This is generally a group with 35 to 40 theoreticians in residence at any one time. It is a very large theoretical group.

We feel very fortunate that we have the Institute for Fusion Studies there. I think we can all be very proud that it is perhaps the Nation's and the world's leading center of fusion theory investigation.

One of the things that the IFS is supposed to do, and I hope we are doing, is to provide an information exchange through conferences, short courses, and exchange programs, both nationally and internationally, and to get the information that we have and other people have out to the community. One of the best ways will be the continual flow of postdoctoral fellows out of this institute and into the national laboratories and the universities.

Finally, there is the engineering program which at the University of Texas has been going on for a long time and has had major funding from the DOE. Recently, this was formalized by the university into a new organization with the catchy name of the Center for Fusion Engineering, which will encompass much of the work and give focus to much of the work that has been done in the past at the university.

There is a strong feeling at the University of Texas that fusion engineering cannot develop as a single engineering discipline itself. It really must develop as something in the melange of the traditional engineering disciplines.

The Center for Fusion Engineering is supposed to provide organization to our present work, and, hopefully, to give some purpose and focus to a growing involvement of a large and major engineering college composed of those traditional disciplines to the fusion program.

We recognize that the National Center for Fusion Engineering, which has yet to be funded, is quite something else.

Thank you.

[The prepared statement of Dr. Drummond follows:]

PREPARED STATEMENT OF DR. WILLIAM E. DRUMMOND

Madam Chairman and Members of the Committee:

I appreciate the opportunity to be here to discuss the university fusion programs and in particular, the fusion program at The University of Texas. This year marks my 25th year in the fusion program. Much of my early work in this field was at General Atomic in the 1950's and the early 1960's where the fusion program was funded by the Texas Atomic Energy Research Foundation which, by the way, is still supporting fusion research, although now at The University of Texas. Looking back over these 25 years, I am struck by the dramatic progress which has been made.

Fusion has been called the most difficult scientific challenge of the 20th Century as well as the key to a 21st Century which is both economically strong and environmentally attractive.

From these hearings, I am sure you will conclude that the DOE fusion program is a well thought out program which is aimed at a distant but clearly visible goal and that it is a program which is moving consistently toward that goal. It is somewhat analogous to a train which after a slow start is moving steadily up a long grade. Indeed, it is sometimes difficult today to recall the unexpected starts and stops of this train when it first began to move in the 1950's. The key to this dramatic transition has been our steadily growing scientific understanding of the basic mechanisms of fusion plasmas. This understanding has grown slowly and with great difficulty over the years, and it has come not just from our largest experiments, but also, from smaller experiments designed to test our theoretical understanding. It has, in fact, come in large measure from the strong theory effort which has always been a part of the U.S. fusion program. Indeed, it is largely because of the understanding which has grown out of the close collaboration between theory and experiment that it is possible today to speak with confidence of reaching scientific feasibility within the next few years.

This is not to suggest that we currently have all the understanding of fusion plasmas required to move on to the development of fusion power. Indeed, it is the expansion of our basic knowledge of fusion plasmas which is driving this program forward. In many ways, this basic understanding is like the engine which drives the train up the long grade -- you can't get there without the engine.

It is in the expansion of our basic knowledge of fusion plasmas that universities have and will continue to play an important role. A number of universities have developed large, strong, and sophisticated programs in fusion research which are making direct contributions to the national fusion programs. The University of Texas is an example of this.

At The University of Texas, we have the Fusion Research Center (FRC), the Institute for Fusion Studies (IFS) and the Center for Fusion Engineering (CFE). All of these have major DOE sponsored programs but draw important support from TAERF as well as industry.

TEXT. Within the FRC is the Texas Experimental Tokamak (TEXT) which is a major tokamak research facility. This facility is specifically chartered by the DOE to provide a high quality tokamak experimental facility to explore a wide variety of fusion phenomena. It is a user facility for qualified researchers from both universities and national laboratories. As a result, a broad and vigorous program with experiments involving scientists from many different laboratories is being carried out. In particular, this facility provides the capability for carrying out experimental programs which, for a variety of reasons, cannot be carried out in the national labs. Indeed, within a national laboratory, the need to focus on a predetermined program often precludes the exploration of new or novel ideas. This is true not only from a programmatic point of view but from an engineering point of view as well, e.g., the toroidal vacuum chamber for Doublet at General Atomic is designed to be opened only once or twice a year for the introduction of new diagnostics, whereas the TEXT facility is designed to be opened for this purpose every two weeks. This facility currently provides unique capabilities for carrying out important basic experiments on such topics as impurity transport, energy transport, and the development of radial electric potentials within Tokamaks. Thus, the TEXT facility with its provision for rapid changeover and rapid introduction of new diagnostics provides, as part of its mission, a unique and vital capability to rapidly explore new and novel ideas.

IFS. The IFS, after a national competition, was chartered at The University of Texas by the DOE as the national center for fusion theory. The intent in creating this institute was to provide a single site which was both large enough and strong enough to attack intensively the broad spectrum of diversified and complex problems which arise in controlled fusion research. These include not only problems of long-range significance which may require the development of new methods for their solution, but also shorter range questions which arise from the major national fusion experimental programs. A central tenet of this Institute is that it is not committed to any particular approach to fusion, e.g., mirror machines, tokamaks, etc. but encompasses the broad sweep of fusion capabilities.

A natural consequence of this organization is that ideas and theories associated with a particular type of fusion device often can be applied to other types of fusion devices. As an example of this, theoretical work at the IFS on mirror machines has been extended to provide a much better understanding of the bumpy torus concept. Further, the same ideas have been again extended to provide a possible means to increase the power density in tokamaks.

An example of the long-range problems which are endemic to almost all of the different fusion devices is plasma turbulence, i.e., microscopic fluctuations within the confined plasma. The plasma turbulence leads to an anomalous increase in the heat loss of confined plasmas and is thus of significant concern to all the prospective fusion devices. At the IFS, a team of theoreticians is attacking this basic long-range fusion problem.

A further purpose of the IFS is to serve as a center for information exchange, both nationally and internationally by hosting exchange visits, short courses, conferences and workshops. An important by-product of these activities is the continual flow of Institute postdoctoral fellows to other institutions and national laboratories.

CFE. The third major organization devoted to fusion research activities at The University of Texas is the Center for Fusion Engineering (CFE) established late last year. We are pleased that Herb Woodson from whom you will hear tomorrow has been chosen as *ad interim* director to organize and develop the center's engineering research program and to recruit the faculty and professional engineering staff necessary to achieve the goals enunciated by the university.

What particularly pleases those of us who have spent our professional lives in the examination of scientific issues of fusion is that the CFE represents the first time a broad-based engineering college has made a serious move to get into the fusion business. As fusion continues its progression from scientific research to practical application, it will need increasing attention from a larger number of the traditional engineering disciplines.

And, when practical applications arrive, fusion installations will be engineered, designed, constructed and marketed by organizations that are building similar energy facilities today. Said another way, fusion engineering is not likely to develop as an isolated discipline, but rather, we can expect fusion engineering maturity to come from the application of already existing engineering disciplines to the problems of fusion technology.

Further, we think it even more helpful if these engineering activities can take place in close proximity to a strong and active fusion science research program such as we have in Texas. This will provide the fusion engineering activity with the substantial scientific input which is still required at this stage in fusion's development.

It also should be said that the breadth of scientific and engineering activities needed to nourish a fusion engineering program at this stage is likely to be found only at a major research university with a strong graduate emphasis.

For all of these reasons, The University of Texas is a fitting host for a major fusion engineering center such as the CFE. It has a large, nationally recognized College of Engineering with active research programs in most of the traditional disciplines. It has a long history of fusion research in both science and engineering, strongly supported by the Texas Atomic Energy Research Foundation, a consortium of the ten investor-owned electric utilities that operate in Texas.

The principal purpose of the Center for Fusion Engineering will be to contribute to the engineering effort to assess, and if possible, prove both the technical feasibility and later the commercial viability of fusion as a source of net energy in useful forms.

TRAINING. It goes without saying that fusion is a program which will reach its goal in terms of the production of electric power, only after a period of some decades. To provide the trained scientists and engineers who will bring this program to fruition is the job of the universities. Because of the DOE support of strong fusion research centers in a number of universities, I feel confident that these universities will be able to provide the required manpower.

Mr. HALL. Dr. Drummond, thank you.

All Members of Congress have access to your testimony. I am almost hopeful that Congressman Pickle, your Congressman, does not find out that he has something else to brag about. [Laughter.]

Dr. DRUMMOND. I must say that he is aware of this because he is responsible for much of its existence.

Mr. HALL. I know of his work and his interest there.

Dr. Dawson, you spoke of the various university roles. Do you feel that if the committee authorized additional funding for the universities that it would be spent in an optimum fashion? Do you think they ought to delegate it or specify the direction of the funding? What are your thoughts along that line?

Dr. DAWSON. Certainly, the universities could use larger funding. I think there should be competition among the ideas.

Many ideas have been reviewed by panels. I know of many that have received good reviews but have never been funded. I think that there are a lot of things that could be supported there. Of course, I think that if DOE had the money to do that they should look over the programs. I would look for those things which I call high leverage, things that might pay off in some reasonable length of time and perhaps reduce the cost of major machines. If you look at the cost of neutral beams or RF power sources, or things like that, there may be better ways to do those things. If somebody has an idea how to do that, that's a potentially high-leverage thing which could save the program a lot of money. That money could then be plowed back in to advance other areas of the research.

Mr. HALL. Something that was of interest to me—you say that no cooperation between the universities and the industries——

Dr. DAWSON. Well, not "no," but it is very small.

Mr. **Hall**. Practically none, then, particularly with the need for industrial involvement? You also alluded to a new office perhaps in DOE. Are you suggesting that ought to be the link? How are you going to go after meeting this need?

Dr. **Dawson**. That is just a suggestion. I do not know whether that is the optimum thing. At DOE there is no real person who is concerned with the university programs. Recently—you or the university fusion association was included in the FPCC.

Dr. **Shohet**. No more.

Dr. **Dawson**. You do not have that?

Dr. **Shohet**. We used to have a representative on FPCC.

Dr. **Dawson**. FPCC, but FPCC——

Dr. **Shohet**. FPCC is defunct, and there is no representative——

Dr. **Dawson**. Therefore, I think the universities actually find rather small representation in DOE. They are somewhat ignored.

Dr. **Drummond**. May I comment on that?

Mr. **Hall**. Yes, Dr. Drummond.

Dr. **Drummond**. Something which I think most people in this room know is that in the middle fifties the investor-owned utilities in Texas formed something called the Texas Atomic Energy Research Foundation, which began supporting fusion research at General Atomic in the middle fifties and continued up through the middle sixties. It is still supporting fusion research, although now at the University of Texas.

This is an example of a nontrivial amount of money, more than $20 million, being put in by industry in hope of helping a long-range source of energy.

We have found very strong response in Texas industry if we go out and talk to them about fusion, not only just the scientific parts but the engineering parts. I think it is a matter of you getting the kind of interaction that you work for.

I know you [UCLA] and TRW aren't exactly at odds.

Dr. **Dawson**. No, we are not exactly at odds, but there are many projects that we proposed and that TRW proposed; ideas that came out of UCLA, and we did not think they were appropriate to do at UCLA because of the size and engineering experience required. Many of us consult there. We proposed that maybe TRW should propose them. These things were proposed and got good reviews, but they were never funded. So those things died.

I think your experience with getting industrial support may be somewhat better than ours.

Mr. **Hall**. We have all that Texas money.

To digress just a moment, Dr. Drummond, I am sorry I did not recognize you as a fellow Texan. I had the pleasure of meeting with Ensign George Gay, one of our fellow Texans, who was the survivor of the Battle of Midway. He told me that he had never asked anyone where they were from because if they were from Texas, they would tell you. [Laughter.]

And if they weren't, there was no reason to embarrass them. [Laughter.]

I do thank you for your testimony, Dr. Shohet, Dr. Dawson, Dr. Drummond.

For purposes of the record and because there is a pending vote on the floor at this time, staff would like to ask some questions to have the answers in the record.

For the record, we will adjourn, and we would ask you gentlemen, if you would, to let staff ask you several questions.

The Subcommittee on Energy Research and Production will meet tomorrow at 9:30 a.m. on electric energy systems, energy storage systems and small-scale hydroelectric systems, in room 2318 at 2 p.m. There will be a meeting on magnetic fusion energy in room 2325.

Gentlemen, thank you.

[Whereupon, at 4:01 p.m., the subcommittee recessed, to reconvene the following day, Thursday, March 17, 1983, at 2 p.m.]

APPENDIX

MC CORMACK ASSOCIATES, INC.
Consultants in Science, Energy and Government

506 A STREET, S.E.
WASHINGTON, D.C. 20003
(202) 544-0254

MIKE McCORMACK
President

15 Apr 83

The Honorable Marilyn Lloyd
Chairman, Subcommittee on Energy
Research and Production
B-374 Rayburn House Office Building
Washington, D.C. 20515

Dear Marilyn

Thank you for your letter of April 12 and for the additional questions that you submitted to me for my consideration. I will try to answer them as follows.

> Question 1. You recommended that FMIT be funded at $10 million for FY 1984, rather than the $3 million level of the Department's request. Do you have any other specific recommendations for added program funding?

Answer: If this question refers only to the FMIT, then my response is that we should actively seek 25% funding from the Japanese and 25% from the European Economic Community each year for an accelerated program to complete the project. $10 million for FY 84 from the United States and $5 million each from the other two contributors would start an accelerated program, and could lead to completion of the project within 2 or 3 years.

If the question above refers to the total fusion budget, then I suggest that additional funds be provided for engineering development and materials testing as early as possible and that the provisions of the Magnetic Fusion Energy Engineering Act of 1980 be observed as much as possible. Funding should be provided to start a Center for Fusion Engineering, and studies related to an engineering test facility should be funded.

In addition, I recommend that the Department be directed to explore support for credible private companies such as INESCO and GA Technology in their research and development for the RIGGATRON and OTHE respectively. I do not recommend spreading money generally around the country to anybody who claims to have a fusion idea, but INESCO and GA Technology are reliable organizations with credible programs, and a great deal of benefit could be provided to our nation by economic support for these projects.

> Question 2. Given that we have fallen behind the schedule called for in the Magnetic Fusion Energy Engineering Act of 1980, what do you feel is a realistic time frame in which we could have a fusion engineering test facility on line?

Answer: I believe that a fusion engineering test facility should go on line in the early 1990s. The basic question that must be asked now is whether it is more practical to plan for a very large expensive machine, somewhat like an enlarged TFTR, or a small cheaper machine such as the RIGGATRON. It may be possible to save a great deal of money by committing to a small machine such as the RIGGATRON for an engineering test facility; and it may be prudent to recognize at this time that small machines -- rather than large ones -- may be the most practical for future commercialization.

Sincerely,

Mike McCormack

Mike McCormack

MMc/cm

GA Technologies

GA Technologies Inc.
P.O. BOX 81608
SAN DIEGO, CALIFORNIA 92138
(619) 455-3000

April 29, 1983

Mrs. Marilyn Lloyd, Chairman
Subcommittee on Energy
Research and Production
B374 Rayburn House Office Building
Washington, D.C. 20515

Dear Mrs. Lloyd:

Thank you for your questions regarding my testimony on the FY 1984 DOE Magnetic Fusion Energy Budget. I am pleased to provide you with the following responses. I hope they prove helpful.

1. Question:

You propose the establishment of a National Ion Cyclotron Heating Engineering Program which would be contracted to industry. Given that Oak Ridge National Laboratory is the lead laboratory for radio frequency heating, exactly what role do you see for industry? Will industry be kept in a strictly supplier role?

Response:

The primary role of industry in the National Ion Cyclotron Heating Engineering Program is perceived as engineering development, specifically designing and fabricating a system suitable not only for the near-term experimental applications but also for use on a first generation tokamak reactor. Oak Ridge National Laboratory, although not officially designated as a lead laboratory, does have an aggressive radio frequency heating technology development program. For the specific frequency range of interest in the ion cyclotron program, the technology for the systems which dominate the program cost is already sufficiently mature that responsibility for these systems could justifiably be made

The province of industry. This responsibility will extend to design and fabrication and, in cooperation with the user facilities, to installation and testing as well. Further advances in technology are essential, however, at the interface between the heating system and the plasma, and it is intended that Oak Ridge National Laboratory play a major role in this element of the program.

2. Question:

The heating program you recommend requires $10 million in budget authority and $5 million in budget outlays during its first year. How large a long-term program would we be buying into -- $20 million, $50 million, or $100 million-plus?

Response:

Doublet III proposes to perform physics and engineering experiments with 5 MW of ICRF at a cost of $15 million. The high-power ICRF is primarily for TFCD.

The present cost estimate for the complete National Ion Cyclotron Heating Engineering Program is $104.3 million. The breakdown by fiscal year for budget outlays is as follows (in $ million):

FY 84	FY 85	FY 86	FY 87	Fy 88
5.0	21.5	28.5	31.0	18.3

Sincerely,

Harold M. Agnew
President

UNIVERSITY OF CALIFORNIA, LOS ANGELES

BERKELEY · DAVIS · IRVINE · LOS ANGELES · RIVERSIDE · SAN DIEGO · SAN FRANCISCO   SANTA BARBARA · SANTA CRUZ

April 29, 1983

CENTER FOR PLASMA PHYSICS AND FUSION ENGINEERING

The Honorable Marilyn Lloyd, Chairman
Subcommittee on Energy Research and Production
Committee on Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Madam Chairman:

This is in answer to your letter of April 12th requesting a supplement to the record of my testimony before your committee in March 1983.

First, with regard to UCLA's funding by DOE for Magnetic Fusion: the total allocated for 1982-83 (up to this date) is 2.893M operating funds and 0.375M capital equipment funds for a total of 3.268M. At present we can only estimate fiscal year 84 funding; we expect it to be essentially the same with perhaps a modest increase for inflation. If I were to give an estimate it would be 3.5M. With regards to our NSF support none of it goes specifically for fusion research. That is not to say that some of the research done on our NSF grants does not have applications to fusion, but our NSF sponsored research concentrates on plasma questions of a fundamental nature or of an exploratory nature which would not generally be supported by the DOE fusion program. Examples of overlapping interests would be basic understanding of plasma turbulence, acceleration of particles by intense waves, generation of radiation from plasma as for example in the free electron laser. It is difficult to pull together exactly our total NSF funding for fiscal 82-83 because the NSF grants do not run by fiscal year but have various starting dates. However, I think you can get a pretty good idea from the attached pie chart of UCLA's plasma funding for fiscal 82. This chart was taken from the "Project Planning Guide for the Plasma/Fusion Energy Program at UCLA-1982" prepared for the UCLA administration. From that chart I would say that NSF funding is slightly less than half that of DOE.

Your question regarding an appropriate level of DOE funding for universities is a very difficult one. While I cannot speak for all universities, I really do not think that a specific amount of money or fraction of the budget should be the criterion. Rather, funding should be determined by what the universities can contribute to the fusion program, although I recognize that this is difficult to define and make quantitative.

Overall, I would say that the present level of funding at UCLA is adequate for our current programs and that we feel fortunate to have this level of support. Increased stability of University funding is urgently needed to attract superior faculty and research staff and for student support (it takes 4-5 years to train a Ph.D.). Multi-year funding grants, such as those used by NSF, would be a great improvement over the present pattern of year-by-year funding.

At UCLA we believe that, over the years, we have had ideas and innovations which could be of great value to the fusion program but it has been hard to get support for these even though they receive good reviews. Some examples are:

1. The Simulated Advance Fuel Experiment (SAFE); an experiment involving fusion in a pure ion plasma which would give valuable data for determining the advantages of polarized fuels.

2. Exploratory experiments to use multi MEV ion beams for plasma heating and current drive. Here we have tried to work with National Labs (Princeton) and industry, but despite strong recommendations by a review panel, nothing has happened. Neither the labs nor DOE have shown any interest in funding university-industry collaboration in this area. Considering the critical importance of efficient, effective heating to the success of fusion reactors and the limitations of all existing forms of heating, I think that exploration of the concept would be prudent.

3. Proposals to develop negative ion beams for heating tokamaks.

4. Proposals for developing free electron lasers for electron cyclotron heating of high field tokamaks and for plasma diagnostics.

5. One of the promises of fusion is that it will be a "clean" souce of energy. To achieve this goal, alloys must be developed in order to solve the radioactive waste problem. DOE has largely concentrated on stailess steel for the last ten years, even though these alloys may pose significant disposal problems. There is very little funding left over for innotvative university research, which could make a large impact on this problem. Even with limited funds, UCLA has developed special vanadium steel allys. With support we feel UCLA could do much better in this area. There are additional aspects of radiation damage to reactor components which we feel are not adequately funded and for which UCLA has proposed broader research programs than are presently funded.

When university scientists have ideas which they believe are important to the fusion program, when these ideas receive good recommendations from panels of experts, and when even exploratory research is difficult to get started, the effect is very bad for morale. It drives good scientists out of the program who then are unlikely to return. Some mechanism for funding innovative ideas for limited lengths of time in the universities would be very valuable. Some such projects might be done in collaboration with industry or national labs.

The budgets I quoted to you do not anticipate funding for any of the ideas mentioned.

Sincerely,

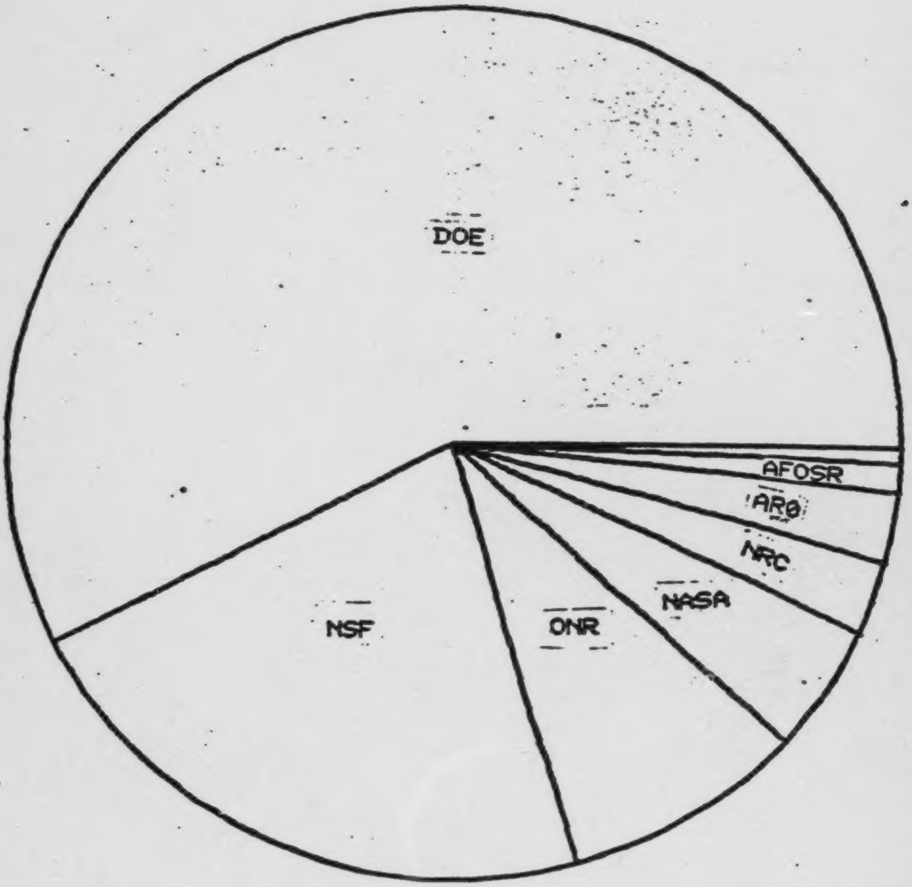
John M. Dawson

John M. Dawson
Director, Center for Plasma Physics
and Fusion Engineering

JMD/meb

UCLA PLASMA PHYSICS & CONTROLLED FUSION PROGRAMS

EXTRAMURAL SUPPORT



FUSION POWER ASSOCIATES
2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879
(301) 258-0545

STEPHEN O. DEAN
PRESIDENT

April 29, 1983

Honorable Marilyn Lloyd
Committee on Science and Technology
Suite 2321 Rayburn House Office Bldg.
Washington, D. C. 20515

Dear Ms. Lloyd:

Thank you for the opportunity to testify to your subcommittee. In reply to your April 12 letter, the following is my response to your additional questions.

Question 1: "You have proposed a $505 million level for the Magnetic Fusion Energy Program in FY 1984--$38 million above the request. What specific recommendations do you have as to where these additional funds are needed?"

Response: As indicated in my oral testimony, I would suggest that the additional funds be designated for the initiation of the "reactor engineering program." As you know, DOE has testified that their new strategy is to carry out a "base program" with existing funds and to postpone a "reactor engineering program," which would implement the fusion act, until an incremental program called the "reactor engineering program" is started later in the decade. One way for Congress to force DOE to implement the fusion act now is to require that any incremental funding by the Congress be used to initiate such a reactor engineering program. DOE should be required to prepare a plan for initiating the reactor engineering program in FY 1984, showing how the incremental funding would be utilized for this purpose.

Question 2: "You recommend that the Committee press for a strong civilian component in the inertial fusion effort. As you are aware, the DOE has requested that the Committee fund activities in heavy ion fusion research. What other activities would you recommend be funded, and at what level?"

Response: The DOE request for heavy ion fusion is intended for studies of accelerator technology. In addition to such studies the DOE should establish a program to perform civilian reactor conceptual design studies for all inertial fusion approaches as well as R&D on "rep-ratable" (i.e., many pulses per second), efficient (approx. 10%) laser and particle beam technologyies. DOE Defense Programs at this time are content to fund single shot, inefficient driver technologies.

Sincerely,

Stew Dean
Stephen O. Dean

"TO TAKE FROM THE SEA AND TO IMITATE THE SUN"

UNIVERSITY FUSION ASSOCIATION

OFFICE OF THE CHAIRMAN
of the Executive Committee
J. L. Shohet

PLEASE REPLY TO:
Torsatron/Stellarator Laborato
Department of Electrical
and Computer Engineering
The University of Wisconsin
Madison, WI 53706
(608) 262-1191

UNIVERSITY FUSION ASSOCIATION
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The Johns Hopkins University
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Naval Postgraduate School
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Northwestern University
Oklahoma State University
Penn State University
Purdue University
Queens College of CUNY
Rensselaer Polytechnic Inst.
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University of South Florida
University of Tennessee
University of Texas
University of Toronto
University of Washington
University of Wisconsin
Washington University (St. Louis)

May 2, 1983

The Honorable Marilyn Lloyd, Chairman
Subcommittee on Energy Research & Production
Committee on Science and Technology
U.S. House of Representatives
Room B-374 Rayburn House Office Bldg.
Washington, D.C. 20515

Dear Mrs. Lloyd:

Thank you for your letter of April 12, 1983. I am pleased to respond to your questions as follows:

1) I have consulted several members of the University Fusion Association concerning your request for some examples of experiments currently in operation at national laboratories which are "of the scale and mission of those normally done at universities". I submit to you the following list:

Experiment	Location
ACT-I	Princeton Plasma Physics Lab.
Spheromak	Princeton Plasma Physics Lab.
RF Test Facility	Oak Ridge National Lab.
Fusion Engineering Design Center	Oak Ridge National Lab.
Liquid Metal Corrosion Studies	Oak Ridge National Lab.
EBT-S	Oak Ridge National Lab.

ZT-40	Los Almos Natl. Lab.
Theta Pinch Experiments	Los Alamos Natl. Lab.
Compact Torus Experiments	Los Alamos Natl. Lab.
Compact Torus Experiments	Lawrence Livermore Natl. Lab.
Wave Propagation Studies	Los Alamos Natl. Lab.
Plasma Gun Studies	Los Alamos Natl. Lab.
Liquid Metal Corrosion Studies	Argonne National Lab.

2) The funding for the systems studies program of the Wisconsin Fusion Engineering Program for the years you have requested is as follows:

FY 1982	$590,000
FY 1983	350,000
FY 1984	350,000 (this number has been given to the Director of the Fusion Engineering Program by DOE)

The activities transferred from this program in this area were given to the Fusion Engineering Design Center at Oak Ridge National Laboratory.

3) The tokapole/multipole program has provided me with the attached graph depicting their funding levels during the past years. I have been advised that the staff of this laboratory has estimated that the fiscal 84 funding request would be $825,000.

	TOKAPOLE	OCTUPOLE
FY 80	$340K op + $35K Eq.	$245K op + $100K Eq.
FY 81	$400K op + $75K Eq.	$395K op + $25K Eq.
FY 82	$450K op + $50K Eq.	$365K op + $60K Eq.
FY 83	$475K op + $50K Eq.	($150K op + $20K Eq.)*
FY 84	Proposal Not Completed	($534K op + $75K Eq.)**

* Promised

** Requested

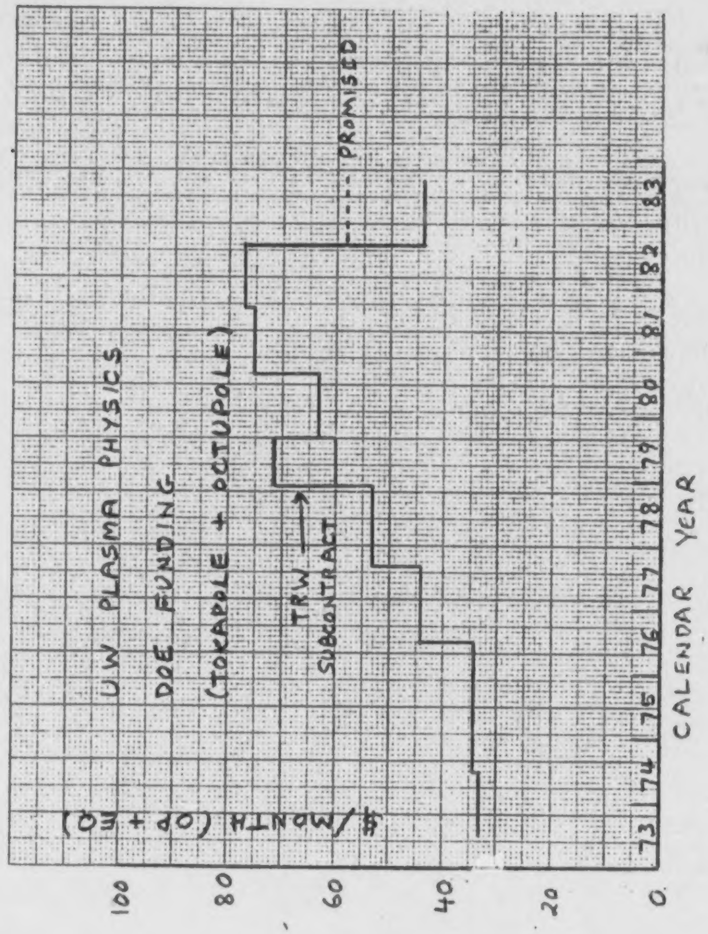
I would also like to clarify my statement in the testimony regarding the percentage of the fusion budget earmarked for Universities. I stated $21 million which is 4% of the total. If MIT is included, the total is then $44 million. Never-the-less, the TARA experiment at MIT was funded there when the MIT budget was considerably lower, so my inclusion of TARA as a University-level experiment still stands.

I hope that this information will be helpful to you. I am greatly appreciative of the kind consideration you and your staff have given to our problems, and I am hopeful that you will be able to assist us in their resolution.

Sincerely,

J. L. Shohet
Chairman

JSL/gr


UW PLASMA PHYSICS
DOE FUNDING
(TOKAPOLE + OCTUPOLE)
TRW
SUBCONTRACT
PROMISED
$/MONTH (OP + EQ)
0
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83
CALENDAR YEAR

1884 · 1984
A CENTURY OF ELECTRICAL PROGRESS

IEEE **TECHNICAL ACTIVITIES BOARD/UNITED STATES ACTIVITIES BOARD**
ENERGY COMMITTEE

6 May 1983

Honorable Marilyn Lloyd Bouquard
Chairman, Energy Research & Production
  Subcommittee
Committee on Science and Technology
U.S. House of Representatives
Washington, D.C. 20515

Dear Madam Chairman:

Enclosed are the answers to the two supplementary questions you had asked me, as the IEEE Energy Committee spokesman, to address in your 12 April 1983 letter. The questions dealt with (1) providing specific recommendations on how new or amended fusion legislation might be directed, and (2) what would be an appropriate level of university involvement in the fusion program.

Thank you for asking me and the IEEE to share our thoughts with you on these matters.

Sincerely,

Allan T. Mense

Allan T. Mense
IEEE Energy Committee
Phone: (314) 576-8253

ATM:lmm

Enclosures

THE INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, INC

Supplementary Question 1

On Page 5 of your testimony, you stated that "new or amended legislation would have a beneficial effect." What specifics would you suggest in amending the MFEEA of 1980?

Answer

Many of the areas covered in the present legislation (PL-96-386) can be incorporated into new legislation. Areas needing little, if any, modification would be Sec. 2(a), 3, 7(a) through 7(f), 10(a), 10(b), 11(a), and 11(b).

The main ingredient in any new legislation should be the establishing of Nationally recognizable goals for the fusion program and the setting of reasonable dates for their completion (Sec. 2(b)).

New legislation might require the department to provide annually an updated "20 year" plan which would show how the aforementioned milestones are being met and at what cost.

If the Congress believes the concept of a Center for Fusion Engineering is still viable, then this should be reinforced.(Sec. 6(a), (b)). Otherwise this section should be deleted.

Finally, one should note that increases in the Fusion program proposed by the Administration for FY 82 through FY 84, have not kept up with funding levels set forth in MFEEA. In fact, when one compares the budget for fusion with those of other programs within OER during that timeframe, one finds

- The Nuclear Physics budget has increased 14%
- The High Energy Physics budget has increased 34%
- The Basic Energy Sciences budget has increased 45%, while
- The Magnetic Fusion Energy budget has increased only 3.5%.

While the IEEE Energy Committee declines comment on organizational matters at this time, one wonders whether the Fusion program is well represented within OER.

Supplementary Question 2

What would be appropriate level of university participation in the DOE Magnetic Fusion Energy Program?

Answer

In the Magnetic Fusion Program within DOE, the funding in the FY 84 request breaks down as shown in Figure 1. It is to be noted that \$45.8/\$467, or 10% of the budget, is spent at universities. Of this, one half (\$23.2 million) is spent at one institution alone. Opportunities for expanded involvement by other universities should be pursued. We do not endorse the concept of R&D as an entitlement program, with portions set aside for specific sectors or geographic regions. We do, however, feel that additional funds could be well spent within the University fusion research community. In order to aid the Committee in their fact finding, we have provided Figure 2, which displays the proportion of the R&D budget committed to universities for a number of Federal research agencies.

FIGURE 1

MAJOR FUSION R&D CENTERS

MAJOR CONTRACTORS	EMPHASIS	TOTAL FY 84 PRESIDENTIAL BUDGET (BA $ IN MILLIONS)	PERCENT
PRINCETON	TOKAMAKS	134.7	29
OAK RIDGE	TOKAMAKS, EBT. TECHNOLOGY	67.0	14
LAWRENCE LIVERMORE	MIRRORS	109.5	23
GA TECHNOLOGIES	TOKAMAK	38.5	8
LOS ALAMOS	ALTERNATE CONCEPTS	21.5	5
MIT	TOKAMAK, MIRROR	23.2	5
OTHERS*		50.0	11
UNIVERSITIES		22.6	5
		$467.0	100%
(TOTAL INDUSTRY,EST.) **		(140)	(30%)

* INCLUDES: ANL, BNL, LBL, HEDL, PNL, INEL, SANDIA, DIRECT INDUSTRIAL CONTRACTS (EXCLUDING GA) AND PROGRAM DIRECTION

** THIS FUNDING TO INDUSTRY IS PRINICPALLY FOR SUPPLYING PARTS & FABRICATING COMPONENTS TO PRINTS.

FIGURE 2

Agencies	R&D $ in Univ. / Total R&D $	Univ. Funding (Millions $)
HHS (NIH)	56% (60%)	2,481 (2,297)
NSF	72%	899
Defense - Military	3%	873
Agriculture	30%	258
ENERGY	6%	273
NASA	6%	193
AID	26%	42
EPA	19%	40
Education	52%	70
Interior	6%	19
Transportation	4%	20
Commerce	6%	14

1984 DEPARTMENT OF ENERGY AUTHORIZATION

THURSDAY, MARCH 17, 1983

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Research and Production,
Washington, D.C.

The subcommittee met, pursuant to notice, at 2 p.m., in room 2325, Rayburn House Office Building, Hon. Marilyn L. Bouquard (chairman of the subcommittee) presiding.

Present: Representatives Bouquard, Winn [ex offficio], Walker, and Chandler.

Staff present: Bill Harvey, minority energy coordinator; Dr. John V. Dugan, staff director.

Mrs. **Bouquard**. The subcommittee will come to order.

Good afternoon, ladies and gentlemen. Yesterday we heard from the fusion community through both an industrial engineering panel and a strictly university group. Their message was that although the program gives us cause for some rejoicing, the budgets with which we are presented are limiting the industry and university roles in the program. There was also some criticism of the national laboratories by the university community and that is an issue we must explore further with the Department.

I am pleased with the progress that has been made in the program, for example, the Christmas Eve first plasma in the TFTR at Princeton. However, I am very disappointed that OMB chose to cut $38 million from the DOE request and it appears that the science adviser's office had a role in that decision. This subcommittee has had serious problems with OSTP meddling in both the fission and fusion programs beginning early in the last Congress.

As regards the subcommittee's relationship with DOE, I am grateful for the willingness of Dr. Trivelpiece and Dr. Clarke to discuss issues in a candid fashion and thereby avoid surprises to both the subcommittee and the DOE. I do hope that this dialogue continues and grows in such a way as to benefit the magnetic fusion energy program.

It is clear that there is a disagreement between the subcommittee and the administration as to the need for an engineering emphasis in magnetic fusion. I believe strongly there must be some engineering development begun in terms of a data base and innovative design work for the next generation of devices. The administration appears to want to constrain the program to a scientific feasibility thrust. As a result, they have flattened the budget so as to limit the flexibility in funding which would maintain the momentum of industry involvement in MFE.

We are happy to have with us the Honorable Pete Stark, one of our colleagues, and Dr. Alvin Trivelpiece. We welcome your appearance before our subcommittee today as in the past. Pete we are very supportive of the work at Lawrence Livermore in the mirror program, and appreciate your taking the time to share your views with us.

Dr. Trivelpiece, welcome, I am looking forward to hearing your testimony and that of the two distinguished advisory groups.

Without objection, our proceedings may be covered and recorded by the media and other persons.

Pete, we welcome your testimony; you may proceed.

STATEMENT OF HON. FORTNEY H. STARK, A MEMBER OF CONGRESS FROM THE STATE OF CALIFORNIA

Mr. **Stark**. Thank you very much, Madam Chairman.

I have presented you, I hope, with a copy of my testimony and I would ask that it be included in the record.

Mrs. **Bouquard**. It will be made a part of the record and you may proceed as you wish.

Mr. **Stark**. I will try, in the interest of your time, to summarize. I want to commend your committee and the work you have done. Your additions to last year's budget helped preserve the fusion effort that is going on in this country. I think last year can be termed a succeess in trying to obtain our goals. The Tokamak fusion test reactor at Princeton was completed, and it was successful in reaching plasma just before Christmas. The magnetic fusion test facility at Livermore has proceeded into its construction phase, and it had a successful test of its ying-yang magnet in February. I would like to, as we get on to Elmo Bumpy Torus, find out who invents the names for these projects because it gives those of us who testify a bit of tongue twisting. But other than that it was a good year.

There were some glitches, however, compared to what we thought would happen in 1980 with the passage of the Magnetic Fusion Engineering Act. Right now all of the fusion programs are suffering because of program cutbacks, and delays. Most importantly, we still haven't received a program plan from the Department of Energy. Now, you may be receiving that today and my testimony will be completely unnecessary, but I do believe this hinders us. The GAO report recently completed for you, Madam Chairman, and to my interests, addresses many of these issues. The proposal submitted by the President this year is an increase from the continuing resolution, but even with this increase, some dramatic cutbacks had to be made.

I think one great loss was the Elmo Bumpy Torus-P project. This project is behind our two lead concepts, tokamaks and mirrors, but it has shown promise. It was one of the few projects being developed with full participation and partial funding from private industry. The loss of this project, I think, means more than just the loss of a promising alternative concept; it cuts out the involvement of private industry. If our ultimate goal is to be commercialization, as

I believe it should be, we ought to be encouraging private industry involvement.

Livermore has gone ahead and brought private industry in on their own. They are scheduled to enter into a contract for the design of specific parts for the MFTF-B very soon and we want to further those goals. I don't think we are encouraging industry to get in if we are going to back off on these projects.

I am concerned, and I know you are concerned, about the lack of a program plan by DOE. I would hate to suggest, as we have had to in other areas with the cooperation of subcommittee chairs, that we make use of the ways provided in our rules for introducing resolutions of inquiry to see such a program plan. There are two alternatives: One, there is a program and we can't see it or there is no program and we can't see it anyway. I think it would be helpful to know what the situation is. If indeed there is a program plan and we can't see it I would like to know why. I think the Chair and the subcommittee would like to as well. I think we need to increase the fusion budget. I am willing to accept the risks that it could all be for naught. Twenty or thirty years from now this testimony may have been all in vain, and the Chair's concern for fusion may have been in error. I think that risk is slight compared with the fact that if we don't go ahead we may miss an opportunity to have safe and clean and reasonably priced energy source for continued industrial production in this country.

I think that the updated program is essential. I think we need to encourage private industries' participation. I don't think that private industry can afford to do this on its own. When I say participation—I think it's preposterous to assume that they could undertake the kind of expentiture necessary to do this by themselves. I think that's precisely why our national laboratories have become a unique national asset. They are able, with a complex of different disciplines and with Federal funds, to undertake that which is in another orbit from private industry.

Mrs. **Bouquard**. I think you have pretty well defined the role of the private sector in the Federal Government. Those are the long leadtimes where you certainly are not going to see a return on your investment for 20 years. I don't think you would want to invest in an investment that wouldn't give you any return for 20 to 30 years. I wouldn't.

Mr. **Stark**. As a matter of fact, you wouldn't want to invest if you were running a company in something where the end result might be something that wouldn't work.

Mrs. **Bouquard**. I don't want to invest in a company that won't be getting any dividends for 20 or 30 years.

Mr. **Stark**. And you don't want to undertake a project where the end result might be to determine that it is so unsafe that you don't want to build it, if you suspected that.

Mrs. **Bouquard**. I also think that the role of the Federal Government is not to, as it has been said, to subsidize the private sector, in development of technologies. But the role of the Federal Government is to see that we have an energy policy and that we have the alternatives in place, not only for our economic survival, for our national security as well. So there, too, is defined the role of the Federal Government.

Mr. **Stark**. I think, Madam Chairman, you have hit the nail on the head. As usual, you perceive these problems long before they mount into massive problems, and as usual you are several steps ahead of me. Right after some minor research problems on the southeast part of this country I know, that second only to your home district, the interests of Livermore, Calif. are foremost in your mind and I appreciate your kind support as in the past.

Mrs. **Bouquard**. I appreciate your interest and your cooperation and I look forward to working with you in this Congress.

Mr. **Stark**. Thank you very much, Madam, Chairman. That concludes my remarks.

[The prepared statement of Mr. Stark follows:]

TESTIMONY OF THE HONORABLE FORTNEY H. STARK, JR. BEFORE THE HOUSE SCIENCE AND TECHNOLOGY SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION.

March 17, 1983

Madame Chairman, it is indeed a pleasure to once again appear before your Committee and address the important issue of magnetic fusion energy research. I want to commend you and the other Members of this Committee on the fine work that you have done in this area. Your additions to last year's budget helped preserve our nation's magnetic fusion effort.

With a few exceptions, 1982 can be termed an overwhelming success in realizing our goals in magnetic fusion. The Tokamak Fusion Test Reactor at Princeton completed construction, and most importantly, was successful in reaching plasma just before Christmas. In addition, the Magnetic Fusion Test Facility in Livermore has proceeded into its construction phase, and last February had a successful test of their large "ying-yang" magnet, one of two that will become a part of MFTF-B.

Unfortunately, however, all has not been bright on the fusion front. Funding, even with this Committee's increases last year, has not been what we envisioned in 1980 with the passage of the Magnetic Fusion Engineering Act, and all of the fusion programs are suffering as a result. Because of these program cutbacks, and project delays, our ability to evaluate comparable alternatives in fusion research has been greatly reduced, and consequently, the next phase reactor has been postponed indefinitely. Most importantly, however, we have still not received a program plan from the Department of Energy. And this, I believe, hinders us the most.

The budget proposal submitted by the President this year is an increase from the continuing resolution, but even with this increase, some dramatic cutbacks had to be made. The greatest loss under the President's proposal is the total elimination of funding for the Elmo Bumpy Torus-P project. Although this project is well behind our two lead concepts, tokamaks and mirrors, it has shown a great deal of promise. In addition, it was one of the few projects being developed with full participation and partial funding from private industry. The loss of this project means more than the loss of a promising magnetic fusion alternative concept; it cuts short an attempt to actively and effectively involve private industry in the development of magnetic fusion energy. If our ultimate goal is to be commercialization of fusion, and I believe it should be, we should be encouraging, not discouraging, private industry involvement.

As the Congressman who represents the Lawrence Livermore National Laboratory, and its employees, I am happy to say that the Laboratory is moving ahead in bringing private industry into the development of the MFTF-B project at Livermore. They are scheduled to enter into a contract for the design and development of specific parts for MFTF-B very soon. This is a giant step ahead, and we should do all we can to further these goals. We are not encouraging industry participation by showing our willingness to cancel important projects like EBT-P.

By eliminating EBT-P we are basically limiting ourselves to the two lead concepts, that is , tokamaks and mirrors, in the decision about the next level reactor we are going to build. And we must keep in mind that even the mirror concept is approximately three years behind the tokamak concept. To adequately assess our alternative options, we should be pursuing them at a much accelerated pace. Therefore, limiting our options at this point would be unwise and self-defeating. As I am sure your know, Madame Chairman, the General Accounting Office has recently completed a draft report for you and me, and this very issue was addressed. GAO states, "..., experts including the National Academy of Science's Research Council, are...questioning whether DOE is adequately pursuing alternative concepts so that options are available when the decision is made to construct the next level reactor." And the report further states,

> Both the National Academy of Science and DOE's Magnetic Fusion Advisory Committee have recommended that the mirror concept be pursued vigorously so that it can provide a development option as the (Magnetic Fusion Engineering Act of 1980) intended. Unless this occurs DOE may be prematurely commiting itself to the tokamak concept for commercial development before it has had the opportunity to adequately assess the advantages and disadvantages of other concepts.

So although the MFTF-B funding is adequate this year, past funding levels have placed it well behind tokamaks. By also cutting, and then eliminating EBT-P, we may have put ourselves into a straight-jacket with regard to our options. I don't think that that helps anyone. If commercialization is to be our goal, we must examine all the alternatives, and move ahead with the best and most promising.

Finally, I believe we have waited long enough for the program plan from DOE for magnetic fusion. This is the third year that the Administration has submitted budgets in magnetic fusion well below those envisioned by the Magnetic Fusion Engineering Act. Obviously program changes have been made, but Congress has yet to receive an updated program plan. The GAO further states in the recent report,

> Because DOE plans to deviate significantly from the fusion energy development strategy set forth in the act, it is particularly important that a comprehensive program management plan be submitted to the Congress. The plan is needed to explain and justify DOE's evolving fusion development program. The plan should also meet the requirements of the act, and should have the flexibility to respond to changing budgetary conditions.

In closing, I want to again thank you, Madame Chairman, and the Members of your Committee for their continued interest and involvement in the research and development of magnetic fusion. Each time I make an appearance here before your Committee we are one step closer to realizing our goal of commercialization of fusion energy, and with that, our own national energy indepedence. Let us not lose sight of those goals after coming so far. Now is not the time to cut back, but the time to actively move ahead. I look to this Committee to guide us to our goals in fusion energy.

Mrs. **Bouquard**. We have included additional funds for the mirror project as well as for increased involvement by the private sector.

Thank you for being with us today and we look forward to counting on you for advice and support this year.

Mr. **Stark**. Thank you very much.

Mrs. **Bouquard**. Dr. Trivelpiece, you may proceed with your testimony.

STATEMENT OF DR. ALVIN W. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY, ACCOMPANIED BY DR. RONALD C. DAVIDSON, DIRECTOR, PLASMA RESEARCH CENTER, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, AND CHAIRMAN, DEPARTMENT OF ENERGY, MAGNETIC FUSION ADVISORY COMMITTEE

Dr. **Trivelpiece**. Thank you, Madam Chairman. You have my prepared statement for the record.

I would like to begin by expressing my regrets that Dr. Clarke could not be with us today. It had originally been planned to have him be the principal witness at this hearing. He is in England and is participating in some of the negotiations associated with trying to make the fusion materials irradiation test [FMIT] project an international facility. You know the International Energy Agency, through its committee on research and development has a fusion power coordinating committee. That committee has been used as the vehicle to bring together a blue ribbon panel to address the questions of what kind of fusion material research needs to be done; whether FMIT is the appropriate facility; and can we forge an international arrangement by which others can be brought into this in a way that we believe the United States can readily support this facility.

However, since John is gone on this important activity at this time, I'm only too happy to be here.

Mrs. **Bouquard**. He's going to stay in England today, is that right?

Dr. **Trivelpiece**. He will be in England for a few days, yes.

Mrs. **Bouquard**. We certainly are looking forward to his return and the results of his trip, which we know will be very productive.

Dr. **Trivelpiece**. I'm sure it will be.

I would like to begin with some remarks and reflect a little bit on some of the things that you just said. The fusion program, we believe, has strong administration support. I think without strong administration support the request would not be $467 million.

You indicated that you thought that the Office of Science and Technology Policy had interfered in, perhaps, a negative way. My view, from a slightly different perspective, is that I think without their help the budget request would not be as much as $467 million. As you know, the administration is trying to favor certain areas of long-range science and technology and the fusion program has strong support.

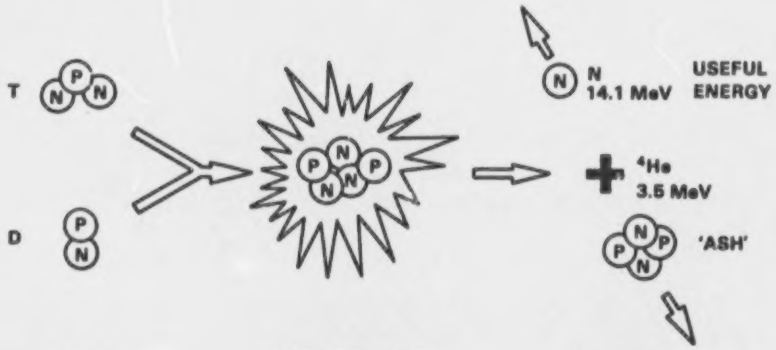
We differ, perhaps, in degree and content of precisely where things ought to be done and some of the directions. But we believe we have tried to outline a balanced program which meets the needs and also helps with respect to controlling the budget deficit and other goals which the President has with respect to the national economy.

To anticipate one question; we are painfully aware of the fact that the comprehensive program management plan is overdue. I believe we have reached the stage in which we have an agreement in principal with most of the parties that have to concur in the final version of that document. I believe that it should only be a routine and mechanical approval process from here out and we should be delivering it in the near future. It is with some embarrassment and apology that I must admit it has taken so long. It should not have, and we would like to get along with it and deliver it to you.

Mrs. BOUQUARD. I appreciate that.

Dr. TRIVELPIECE. I would now like to just go over a few highlights of the program before going to questions.

THE ELEMENTS OF THE FUSION PROCESS AND THE RESULTANT TECHNICAL PROBLEM AREAS



FUEL

- MANAGEMENT OF D&T
- REFUELING TECHNIQUES

REACTION CONDITIONS (DENSITY, TEMPERATURE, TIME)

- PLASMA HEATING PHYSICS AND TECHNOLOGY
- PLASMA CONFINEMENT PHYSICS
- MAGNET AND VACUUM SYSTEMS DEVELOPMENT

PRODUCTS

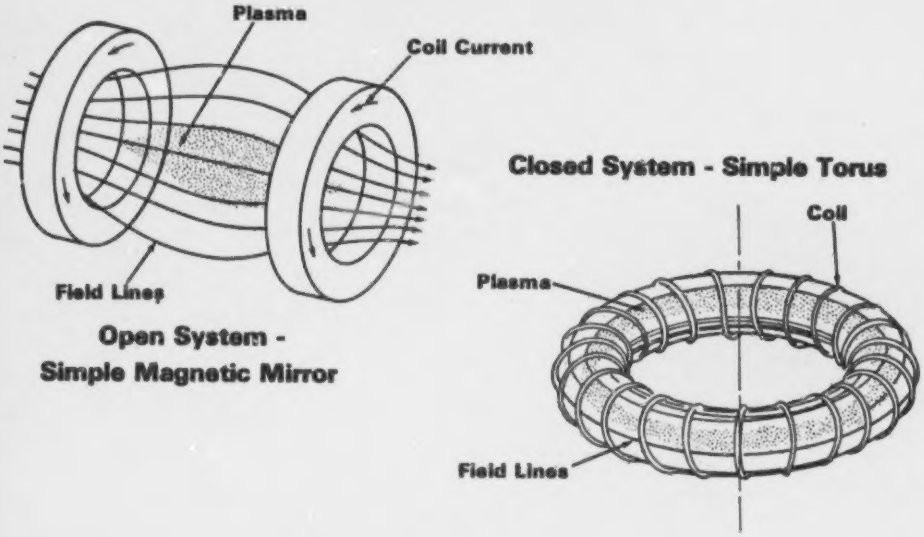
- HEAT TRANSFER DESIGN
- ASH REMOVAL METHODS
- MATERIALS DEVELOPMENT

SLIDE 1

The first Vu-Graph (slide 1).

This is more or less a traditional Vu-Graph, which I'm sure you have seen before. I wasn't sure how many new members of the committe might be here today. The point is that there is an isotope of hydrogen, deuterium, and another isotope of hydrogen, tritium, which, if they can be brought together at high enough speed, will fuse and release energy. This is the microscopic process upon which the macroscopic fusion energy delivery systems are based.

THERE ARE TWO BASIC MAGNETIC FIELD CONFIGURATIONS FOR CONFINING PLASMAS



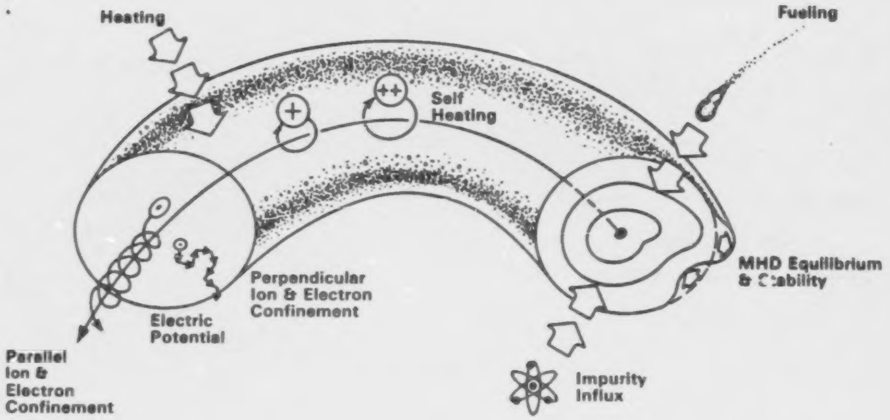
Open System - Simple Magnetic Mirror

Closed System - Simple Torus

Slide 2

The next Vu-Graph (slide 2) shows the schematic approach by which advantage is taken of the fact that charged particles move in circular orbits in magnetic fields. One approach, which you can use to confine high temperature plasma that could result in useful fusion reactions is to have two current-carrying coils of wire that produce the field configuration, depicted schematically on the left. The particles bounce back and forth between the regions where those two coils are, hence the name magnetic mirror. That has evolved over the years into a very sophisticated system, known as the MFTF–B device. The other strategy for containing particles is to have the field lines go around in circular orbits and close on themselves. The configuration depicted on the right is a simple torus. It, of course, is evolved into the Tokamak, which has various other features associated with it.

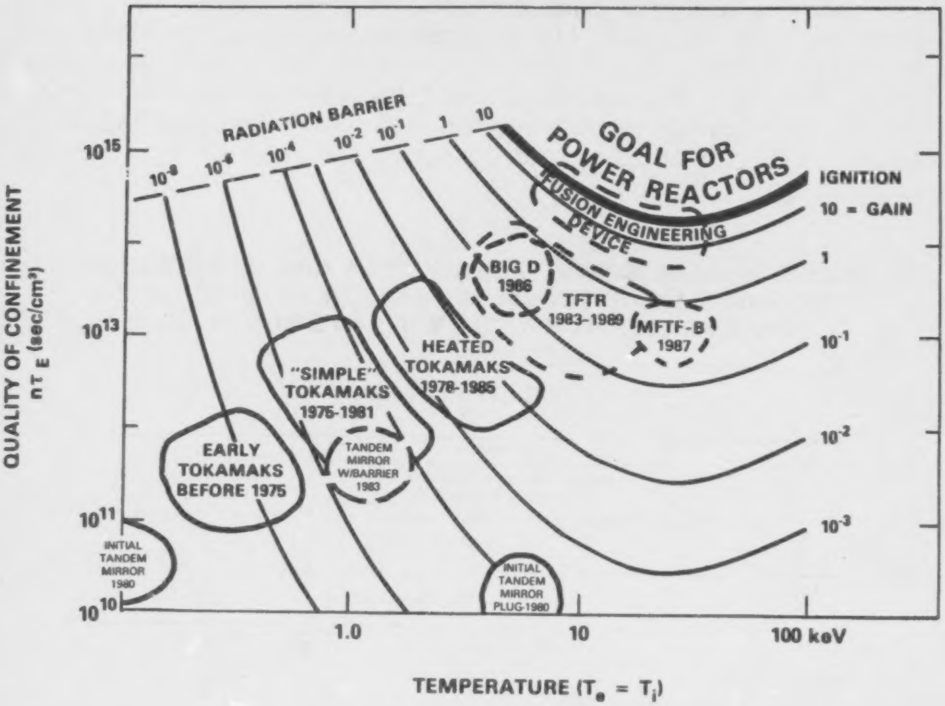
THE EIGHT FUSION PHYSICS ISSUES MUST BE ADDRESSED AS A COMPLETE PLASMA SYSTEM


Heating
Fueling
Self Heating
MHD Equilibrium & Stability
Perpendicular Ion & Electron Confinement
Electric Potential
Parallel Ion & Electron Confinement
Impurity Influx

SLIDE 3

The next Vu-Graph (slide 3) depicts a collection of complicated physics problems. It illustrates that there are a number of problems in the scientific area that need to be addressed in trying to understand how plasmas behave in a system that would be appropriate for fusion reactors. I won't go over all of them. I only want to call attention to one central item. There is a small circle with two plus signs in it, and an arrow associated with it. That stands for self-heating. That is there to depict schematically the fact that, as plasma temperatures are raised to the point that there is energy release coming from the fusion process, eventually alpha particles will begin to deposit heat into the plasma. The next major step in the program is to try to achieve conditions in the plasma where the amount of heat that is being delivered from the fusion process itself begins to beneficially modify the plasma. This will come up in some of the remarks that Dr. Davidson will make when he gives his review of the Magnetic Fusion Advisory Committee report. Otherwise, the point of this Vu-Graph is to illustrate that you can't simply treat each of these plasma problems associated with plasma confinement one at a time in an independent manner. You have to treat them all together in a coordinated fashion.

Increasing Achievements and Facility-Based Expectations Underly the Program Strategy



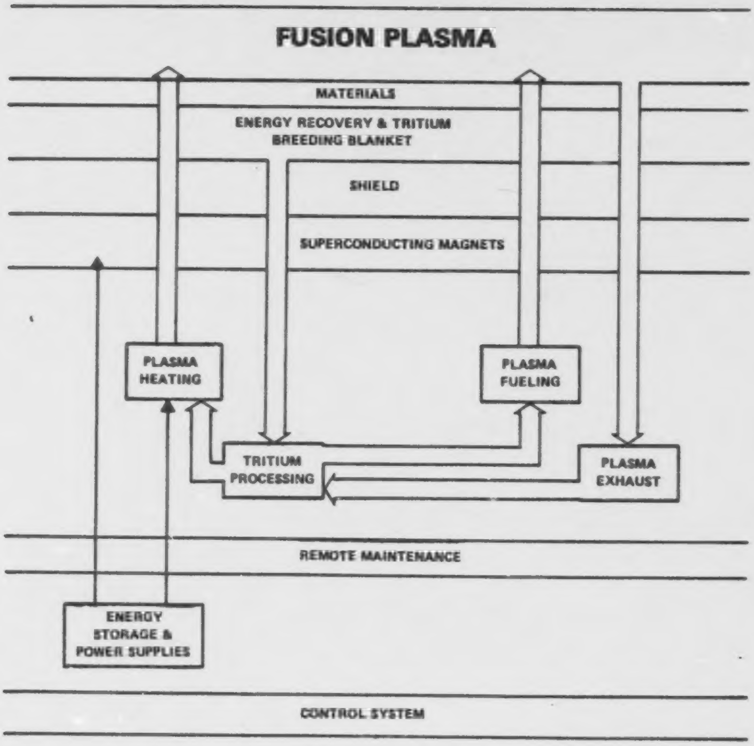
SLIDE 4

The next Vu-Graph (slide 4) is one which we generally show each year. It provides a means by which the people who do research in this area keep score on progress. The quality factor which is on the left-hand axis is plotted against the temperature of the plasma. You can see, starting from the lower-left and moving toward the upper-right, that you go from some of the early experimental devices to a region, if it can be achieved, where a greater energy release will occur than was invested in having brought the plasma together.

It shows the various devices that are currently being operated or planned. You can see the TFTR which has just operated as well as MFTF-B which will be completed in the future, as well as the Big-D, which is the doublet experiment at GA Technologies. The goal is to reach the region where energy-producing reactions take place. I think one of the things that you will hear today, from Dr. Davidson, will be some remarks on the need for an intermediate device between the present collection of experiments and the fusion engineering device. This device would be capable of exploring that aspect of heating plasma by the plasma reactions themselves that I referred to in the earlier chart. We will need to review this device in our fiscal year 1984 program.

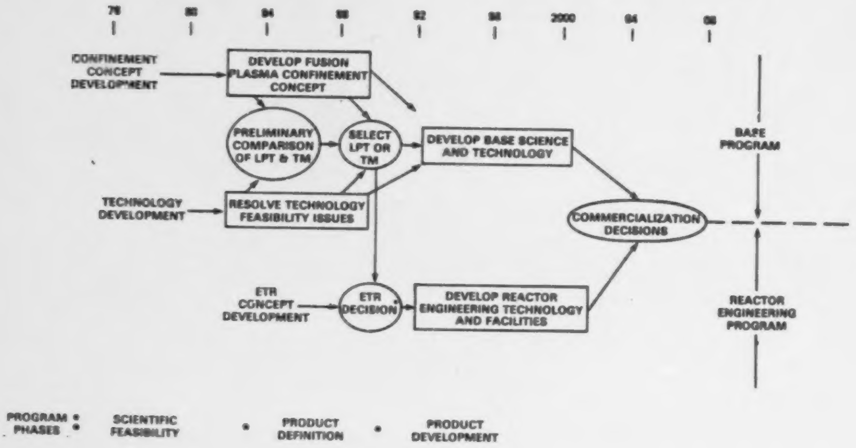
The comment I made earlier about plasma was that you have to look at all of the problems of confinement together. You can't look at heating in one place and confinement in another place and stability in another place. You have to look at them all together. However, the next Vu-Graph (slide 5) shows that the technology problems have a different characteristic. That is various technology areas, such as materials, energy recovery, shielding, breeding, and so on, can be looked at separately. Those can all be addressed separately and then the results brought together at the end. Consequently the strategy for working on technology development is different than the strategy for working the science part of the plasma-physics problem.

UNLIKE PLASMA ISSUES, TECHNOLOGIES CAN BE ADDRESSED INDIVIDUALLY—BUT MUST ULTIMATELY BE INTEGRATED INTO A PRACTICAL SYSTEM WITH A FUSION PLASMA


FUSION PLASMA
MATERIALS
ENERGY RECOVERY & TRITIUM BREEDING BLANKET
SHIELD
SUPERCONDUCTING MAGNETS
PLASMA HEATING
PLASMA FUELING
TRITIUM PROCESSING
PLASMA EXHAUST
REMOTE MAINTENANCE
ENERGY STORAGE & POWER SUPPLIES
CONTROL SYSTEM

SLIDE 5

THE FUSION PROGRAM STRATEGY CONSISTS OF A BASE PROGRAM AND A REACTOR ENGINEERING PROGRAM PURSUED IN THREE SEQUENTIAL PHASES



76 80 84 88 92 96 2000 04 08

CONFINEMENT CONCEPT DEVELOPMENT

DEVELOP FUSION PLASMA CONFINEMENT CONCEPT

PRELIMINARY COMPARISON OF LPT & TM

SELECT LPT OR TM

DEVELOP BASE SCIENCE AND TECHNOLOGY

TECHNOLOGY DEVELOPMENT

RESOLVE TECHNOLOGY FEASIBILITY ISSUES

COMMERCIALIZATION DECISIONS

BASE PROGRAM

ETR CONCEPT DEVELOPMENT

ETR DECISION

DEVELOP REACTOR ENGINEERING TECHNOLOGY AND FACILITIES

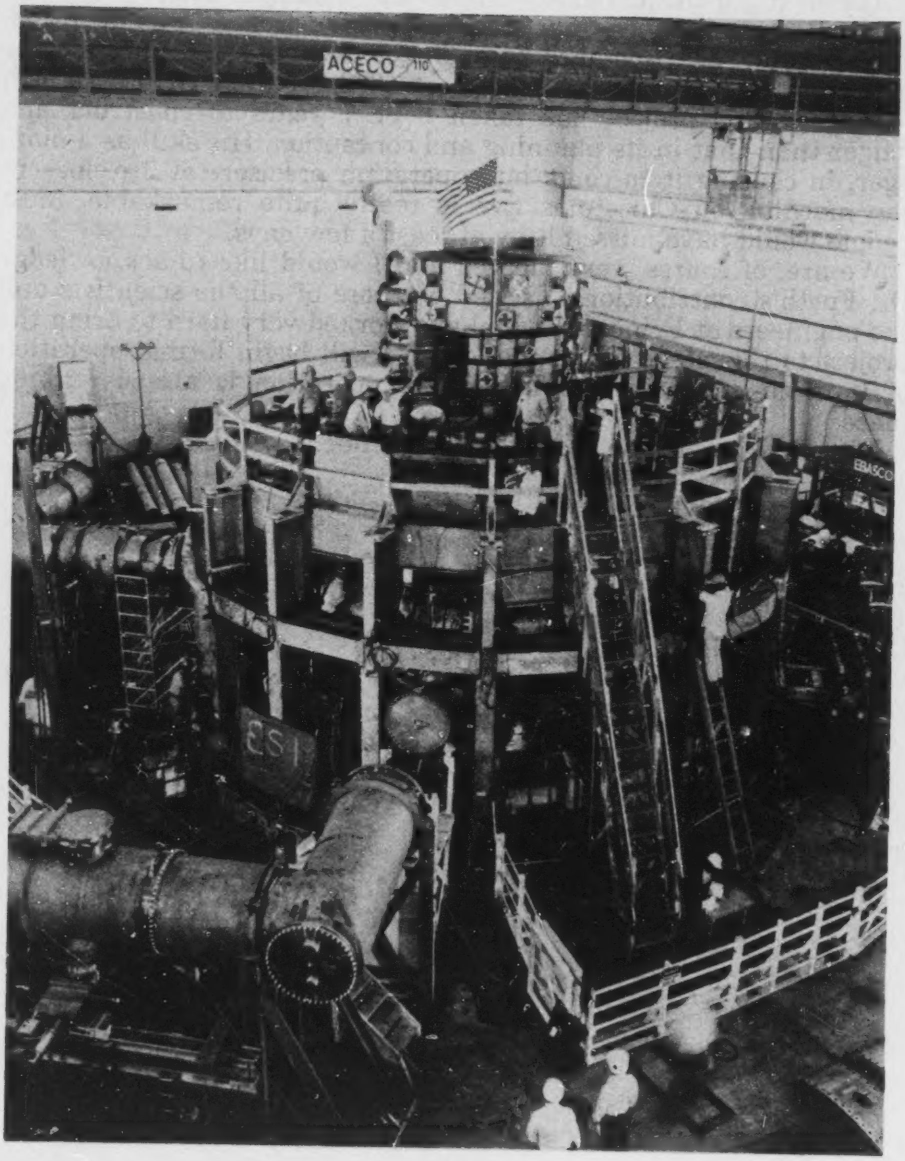
REACTOR ENGINEERING PROGRAM

PROGRAM PHASES • SCIENTIFIC FEASIBILITY • PRODUCT DEFINITION • PRODUCT DEVELOPMENT

SLIDE 6

The next Vu-Graph (slide 6) provides a road map of program organization which is also somewhat conventional. However it does change from time to time and this one is different from some of those that you have seen in the past. It illustrates the fact that there are two pieces to this program. There is a base program and there is the reactor engineering program. In the upper part, and to the left, what you see are those things that we are doing now in the way of a science and technology program that seeks to understand what the plasma confinement concept is going to be, and how we will come to some resolution of the technology aspects of the program that need to be explored. Those will come together at some stage in the future. At that time, if it is appropriate, and I stress appropriate because it may not be. We can make some selection between Tokamaks or mirrors. It has been normally assumed that this program keeps focusing down to where eventually there is a single winner and that that single winner then will march forward into the sunset carrying with it the entire burden of success. Science programs and technical activities sometimes don't tend to go that way. I think there has been a premature or unwarranted fixation on the absolute desire to say that at some predetermined point in time you must make a selection and that tends to create a winner/loser syndrome. I think that's an unfortunate characterization of things. Both elements of this competition may be valuable. Nevertheless, at the moment the intention would be to try to drive things in a direction to come to some conclusion with respect to picking one over the other. The various other alternates serve as contributors to the technology base program.

The engineering program is illustrated in the lower part of that chart along with the engineer test reactor that is part of that program. The timing of this program with respect to the science and technology part is somewhat more flexible than that chart might indicate. I think that is to some extent the debate and dispute that we have been having over how rapidly and in what manner the engineering and development activities should proceed. It could be done in parallel with the science program that's going on now, or it could wait until a later stage when the science is better understood.

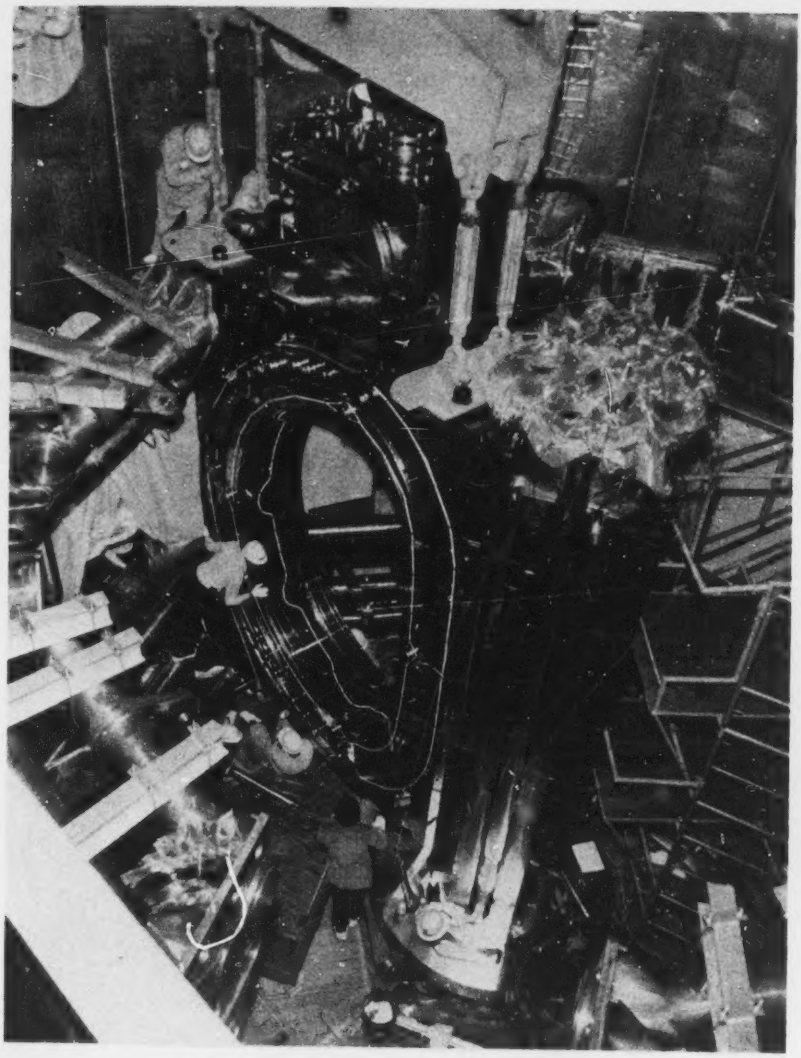

ACECO
110
EBASCO

Slide 7

The next Vu-Graph (slide 7) shows the Tokamak fusion test reactor at Princeton in its final stage of construction. It was operated, as you know, on December 24 at 3 o'clock in the morning for the first time. I think this is a testament to the skill of Dr. Furth as a manager. This project has taken 6 or 7 years to construct and longer than that in its planning and conception. His skill as a manager, in causing it to come into operation precisely at 3 o'clock in the morning on Christmas Eve is really quite remarkable. Most people would have missed it by at least a few days.

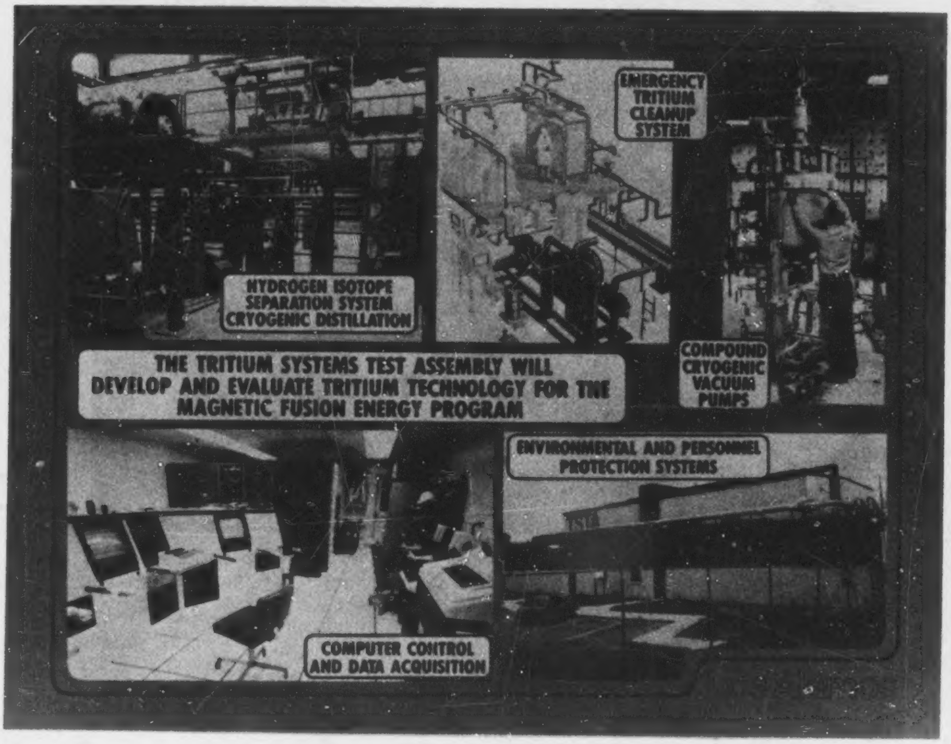
We are, of course, very pleased and I would like to acknowledge Dr. Furth's contributions, as well as those of all the scientists and the engineers at Princeton who have worked very hard to bring the project to this stage of completion. We will begin formal operation of it in the near future. The initial operation is already taking place.

Mrs. BOUQUARD. We share your gratitude.



SLIDE 8

Dr. **Trivelpiece**. The next Vu-Graph (slide 8) shows the large-coil test facility with the coil that has been delivered by the Japanese being lowered into place. I chose this picture to illustrate two things: one, that we have made progress and are nearing completion on this large-coil test facility. The other is that it is quite frequently stated that the fusion program doesn't involve very much in the way of technology development. I think this picture illustrates that technology development is at least a reasonable component of the program when you see activities of that sort being carried out. This has also been a very successful activity. It involves an international arrangement under the International Energy Agency. There are three coils that are being delivered by U.S. vendors and three coils that are being delivered by foreign vendors; the Japanese, the Germans, and the Swiss are each contributing a coil. I think the coils cost something on the order of $10 million each. This will be a test facility to understand the forces and constraints associated with using superconducting magnets in fusion research and development.

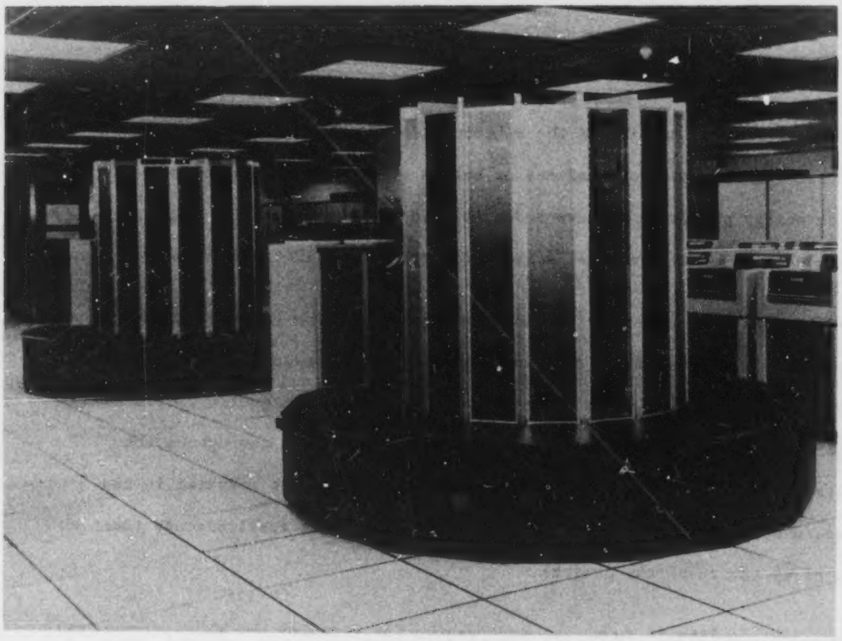


THE TRITIUM SYSTEMS TEST ASSEMBLY WILL DEVELOP AND EVALUATE TRITIUM TECHNOLOGY FOR THE MAGNETIC FUSION ENERGY PROGRAM

EMERGENCY TRITIUM CLEANUP SYSTEM
HYDROGEN ISOTOPE SEPARATION SYSTEM CRYOGENIC DISTILLATION
COMPOUND CRYOGENIC VACUUM PUMPS
ENVIRONMENTAL AND PERSONNEL PROTECTION SYSTEMS
COMPUTER CONTROL AND DATA ACQUISITION

Slide 9

Fusion systems will use the isotope of hydrogen, which is called tritium. It is, of course, radioactive and some care must be exercised in its handling. The character and quantity of the tritium that is involved in these experiments is such that some development of the techniques needs to be done in advance. This Vu-Graph (slide 9) shows several photographs of the completed tritium systems test assembly at Los Alamos Laboratory. It is now in operation and will be used for exploring various aspects of tritium handling.



SLIDE 10

The next Vu-Graph shows a picture of a couple of computers. There is nothing remarkable in the picture itself since computers are available everywhere. These are two Cray-I computers. They are located inside the Magnetic Fusion Computer Center at Livermore. I show them because the fusion program has been a pioneer in certain aspects of computer utilization. The two Crays at the center utilize a time-sharing system that was developed specifically for Cray by the scientists at Livermore. It is important to recognize the kind of interchange that occurs in this sort of an activity. The availability of these kinds of devices, together with the skills of the scientists at Livermore, has produced a system for operating these computers that has now been adapted by a number of other organizations, including the Cray Computer Co. itself.

Madam Chairman, that completes the introductory remarks I had planned to make. I would be pleased to answer any questions you might have.

[The prepared statement of Dr. Trivelpiece follows:]

Statement of Alvin W. Trivelpiece
Director
Office of Energy Research
Department of Energy
Energy Research and Production Subcommittee
of the
House Committee on Science and Technology
March 17, 1983

Introduction

Madam Chairman and Members of the Subcommittee, I am pleased to be here today to testify before the Subcommittee on behalf of the Administration's magnetic fusion energy program. My purpose is to bring you up-to-date on magnetic fusion program matters as they affect our Fiscal Year 1984 budget request.

This year has been been eventful for the Magnetic Fusion program. It has been a time of re-examination of the fundamental program management assumptions under the combined influence of technical progress, the Magnetic Fusion Energy Engineering Act of 1980, and the general budget situation throughout the Federal Government today. This serious re-examination has resulted in new ideas, new expectations, and new management actions. I shall discuss these as part of my review of the program.

First, I would like to summarize our reasons for carrying out this program and present our strategy for bringing it to a successful conclusion. The Federal Government's support of the fusion program is predicated upon the need for the fusion energy option in the Nation's energy future, as well as the near-term benefits associated with developments on the frontier of technology. As a long-term energy option, magnetic fusion energy has characteristics which could allow it to become a principal energy source in the next century. The attractive characteristics include an inexpensive, vast, and secure fuel reserve, as well as environmental and safety aspects that appear to be desirable. While electricity is an obvious end use for fusion, other possible uses include breeding fissile material for fission plants, generating high grade heat for industrial applications, and the direct production of synthetic fuels.

In the near-term there is also a benefit from high technology developments flowing from the fusion program. Fusion program needs drive development in technology areas as diverse as superconducting magnets, high power accelerators and radio frequency systems, advanced materials development, and mathematical methods for computers. Fusion research also makes important contributions in advancing electronics, computer control systems, optics and many other areas. I will highlight some of these current benefits later.

The Department of Energy manages the Government's program that seeks to demonstrate the scientific and engineering feasibility of nuclear fusion. The Department's intent is to maintain the U.S. leadership role in the area of magnetic fusion energy research and development. This is being done by conducting a balanced scientific program of research involving various fusion plasma confinement concepts. During the next decade, it is expected that one of these plasma confinement concepts will be selected for further development as a power reactor core. The Department also plans to develop the magnetic fusion energy technological base and transfer it to industry in preparation for the practical application of fusion.

Program Strategy

The goal of the Magnetic Fusion Energy Program is to develop the scientific and technological information required to design and construct magnetic fusion power systems. The strategy for achievement of this goal has three sequential program phases: scientific feasibility, product definition, and product development.

Scientific Feasibility - The tasks within the scientific feasibility phase are to resolve basic scientific issues for the purpose of demonstrating scientific feasibility and to identify technology issues for the purpose of planning the minimum cost technology development program.

During the past three decades, the program has focused on the basic scientific and technological issues associated with the production, heating and confinement of high temperature plasmas in various magnetic field configurations. This phase of the program is entering its final period with the completion of the Tokamak Fusion Test Reactor (TFTR) facility at the Princeton Plasma Physics Laboratory that is designed to demonstrate the scientific feasibility of magnetic fusion energy release. The experimental program leading to this demonstration is expected to achieve energy breakeven by about 1986; i.e., the generation of as much fusion energy as was required to heat the fusion plasma.

Product Definition - The idea of a product definition activity, is to conduct a science and technology development program to produce a fusion concept which is attractive from the viewpoint of practical application. A clear definition of such a concept before extensive engineering development is central to our strategy. However, there are necessary technological development tasks as well as purely scientific ones in this phase. The basic tasks are to conduct applied scientific research and develop base technology, since both are necessary in order to select a fusion plasma core suitable for practical application. An additional important technology task in this phase is to resolve selected technology issues critical to the subsequent demonstration of engineering feasibility.

We believe that the only realistic candidates for consideration in the 1980's product definition phase are those based upon the technically strongest existing approaches, namely the long-pulse tokamak and the tandem mirror. In the competition between these concepts, we will strive for significant improvements, such as higher efficiency, simpler designs and smaller size. As in the past, we will provide stimulation for this improvement process by supporting research on alternative approaches to the solution of major problems. Selection between the two leading concepts is planned after both have explored reactor-level plasma performance. This exploration should have occurred by about 1988. This selection process is intended to allow effective use of program resources through focused development of the most promising confinement concept in the future. It should also provide an improved confinement concept choice for the power producing core of an advanced engineering device.

The design of an engineering development device for fusion will be initiated during the product definition phase as a planning element in preparation for a subsequent reactor engineering program. This engineering development program, based on the selected concept, is intended to demonstrate the engineering feasibility of fusion. Reference preconceptual designs for the engineering device or devices will be maintained and periodically updated during this phase to incorporate pertinent advances.

The deliverable result of the product definition phase is planned to be the specification of the scientific and technological characteristics of an attractive fusion reactor core (i.e., confinement concept, critical technologies and plasma parameters) as well as a more realistic estimate of its cost.

Product Development - With the selection of an appropriate fusion plasma confinement concept, planned for the end of the decade, the third phase, product development, could begin. The tasks in this phase would be to optimize the selected fusion reactor plasma concept and to demonstrate necessary reactor technology both to demonstrate engineering feasibility, and to initiate engineering development programs. This systematic approach is expected to provide the foundation of scientific and engineering knowledge for private sector decisions on fusion's commercial application.

Program Management

A fundamental concern in implementing this strategy involves the determination of the minimum program size to accomplish the various tasks outlined earlier. We have defined a base program as described in our proposed budget. This program contains the elements needed for demonstration of scientific feasibility, selection and development of a fusion plasma core, development of technology to support the science efforts, and exploring a number of longer term critical technological feasibility issues. These activities are fundamental to any additional engineering development initiatives. They also form an irreducible core if we are to maintain an international leadership role for the United States in this frontier area of technology. As defined, this base program can proceed on a fairly steady budget toward a long-term goal of readiness for commercialization decisions.

Once the base program has successfully defined a suitable reactor concept and a technology base, a complementary reactor engineering program could be initiated to accelerate the translation of this technical information base into a practical application of fusion energy. In our strategy, the increased effort required for such a reactor engineering program would be applied with a high degree of certainty to the specific needs of the engineering feasibility demonstration. This base science and technology program, complemented by the reactor engineering program, could result in readiness for private sector decisions to commercialize fusion in the United States around the turn of the century should scientific, economic, and budgetary conditions allow.

An important element of the base program is research into confinement concepts that offer new and different insights into fusion science. The "alternate" concepts, which are currently the bumpy torus, reversed field pinch, and compact toroids, will be pursued as part of the technical base. They will be supported to provide challenge and stimulation to the mainlines through exploration of novel approaches to common magnetic confinement fusion problems, rather than as "dark horse" reactor candidates. This approach is consistent with the views of the Magnetic Fusion Advisory Committee based on their review of the present alternate concepts.

The use of international cooperation to maintain a coherent base program at minimum cost will be most effective when the U. S. exercises a leadership role in magnetic fusion development. The factors constituting international leadership are maintaining a climate of innovation in a comprehensive scientific program integrated closely with technological efforts to form a balanced research and development program. The United States program has developed this form of international leadership and intends to maintain it in the two major confinement lines and in the development of critical technologies. Nonetheless, we recognize that, in certain areas, progress can best be made through a carefully formulated and managed policy of close international cooperation to share specific tasks.

The appropriate level of industrial involvement in fusion has been a subject of much discussion. In this area, it is helpful to refer to the clear views of a recently convened National Academy of Sciences/National Research Council (NAS/NRC) study on fusion engineering needs. There are two pertinent views. One is that "The magnetic fusion program currently needs the strengthened technological base and enhanced attention to economic viability that industry can provide." The second view is that "Because profitmaking industry is not likely to invest substantial amounts of capital in fusion in the foreseeable future, this involvement will be largely through Government-funded contracts, whether for the provision of components, engineering and managerial services, or personnel."

We plan to maintain our substantial industrial participation in the next phases of the program. Although the industrial participation takes many forms, from supplying hardware to performing development work, the area of systems studies work is especially important because of its breadth and potential for valuable information transfer to and from industrial personnel.

The system studies work to develop conceptual reactor designs for the leading concepts, as well as system studies necessary to prepare for the selection between long-pulse tokamaks and tandem mirrors, have now been placed under a single OFE manager for all reactor studies and designs. The Fusion Engineering Design Center (FEDC), which is an industrially based, central engineering design resource at the Oak Ridge National Laboratory, is under his purview. The FEDC will perform design studies for each concept, receive input from the concept advocate groups at Princeton Plasma Physics Laboratory and Lawrence Livermore National Laboratory, and perform system normalization studies.

OFE will use an industrial contractor to establish evaluation criteria and normalizing standards, assess the design studies against these criteria and standards, and review the research and development plans from the studies. We believe this approach is consistent with the recommendations on the effective use of industry made in the National Academy of Engineering report on the Future Engineering Needs of Magnetic Fusion.

The fusion program does have substantial industrial participation with approximately 30 percent direct funding to industries. This participation in the base program would grow significantly in a reactor engineering program. Eventually, the program sponsorship would shift to private sector sources as commercialization decisions are made, but that point is decades into the future. With the program outlined above, the Department is meeting the intent of the Act, that is to pursue fusion energy research and development vigorously to a point, around the turn of the century, where decisions on commercialization can be made on a firm base of technical information.

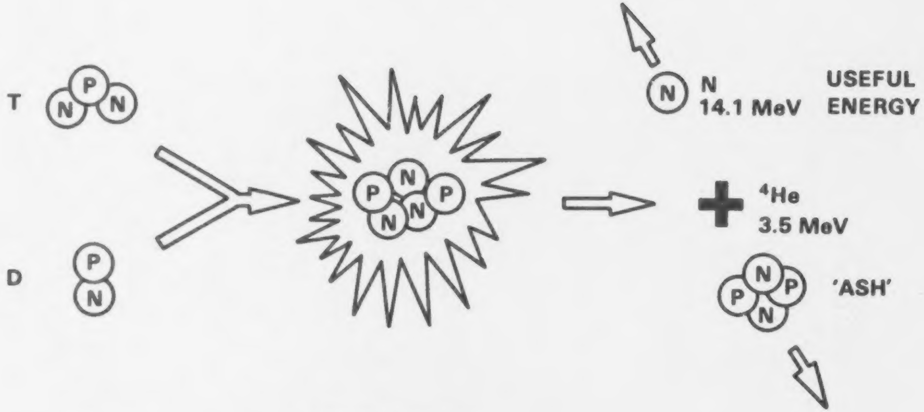
Program Content

Fusion reactions are difficult to produce because the nuclei are positively charged and, thus, repel each other. At sufficiently high pressures and temperatures, however, the nuclei of light atoms collide violently with each other causing the nuclei to fuse and, in the process, to release energy. Attaining the collison speed necessary to induce fusion requires a temperature of 100 million degrees Celsius. Figure 1 illustrates the basic elements of the fusion process, and shows the related scientific and technological problem areas that must be addressed in the program. Just as the fusion reaction, itself, is an irreducible phenomena, these scientific and technological problem areas define the dimensions of an irreducible base program needed to exploit this reaction.

The term "magnetic fusion" stems from the fact that the plasma (a gas so hot that the electrons are stripped from the atoms) can be contained by magnetic fields. The two leading magnetic field configurations are closed and open systems. In a closed system, the magnetic lines of force circle in on themselves forming a doughnut or a toroidal shaped confinement region. The most developed closed system is called the "tokamak". In the open confinement system, magnets are placed near the ends of the system to squeeze the magnetic lines of force. This reflects particles back toward the center of the system, rather than allowing them to escape out the ends. The leading example of the open confinement configuration is referred to as a "tandem mirror".

Research on both the open mirror and closed tokamak systems is aimed at making the plasma hot and dense enough and confining it long enough so that many

THE ELEMENTS OF THE FUSION PROCESS AND THE RESULTANT TECHNICAL PROBLEM AREAS


T
P
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D
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N
P
N
P
N
N
N
N
14.1 MeV
USEFUL
ENERGY
^{4}He
3.5 MeV
N
P
P
N
'ASH'

FUEL

- MANAGEMENT OF D&T
- REFUELING TECHNIQUES

REACTION CONDITIONS (DENSITY, TEMPERATURE, TIME)

- PLASMA HEATING PHYSICS AND TECHNOLOGY
- PLASMA CONFINEMENT PHYSICS
- MAGNET AND VACUUM SYSTEMS DEVELOPMENT

PRODUCTS

- HEAT TRANSFER DESIGN
- ASH REMOVAL METHODS
- MATERIALS DEVELOPMENT

fusion reactions occur before the hot plasma leaks away. Energy breakeven is defined as releasing as much energy from fusion reaction as it takes to heat the plasma. Although practical fusion systems will need to exceed energy breakeven by a factor of ten to one hundred, scientific feasibility constitutes an important milestone in the program. It will be demonstrated when sufficient energy confinement, in a potentially useful magnetic configuration, is achieved to allow energy breakeven.

The United States magnetic fusion program has grown from a small scientific research program in the 1950's to the present goal-oriented research and development program. The early program consisted mainly of small-scale experiments to investigate plasma confinement in a number of magnetic field configurations. These various magnetic field configurations have come to be called plasma confinement concepts. These early experiments served to develop the science of plasma physics so that old confinement concepts were abandoned and new ones developed on the basis of new scientific information. With this scientific progress, the relationships between the superficially different confinement concepts became more apparent. Today we have an integrated program of research utilizing the various magnetic configurations to establish scientific principles which will ultimately allow construction of a practical magnetically confined reactor core.

In the 1970's progress was such that the program was able to produce and study near-reactor plasma conditions in certain magnetic configurations. Presently, the tokamak is the most developed confinement concept, followed closely by the recently developed tandem mirror. The tokamak concept is ready to be tested by attempting to produce reactor-grade plasmas at the existing Tokamak Fusion Test Reactor (TFTR). Production of reactor-grade plasmas using the tandem mirror concept will be attempted at the Mirror Fusion Test Facility (MFTF-B) when construction is completed. Partly as a result of progress in these programs, considerable improvement has also been made in plasma confinement using other magnetic configurations. As noted above, research on these concepts (e.g., the Bumpy Torus, Reversed Field Pinch, Compact Toroid, and Stellarator) contributes in turn to the basic understanding of the behavior and control of high temperature fusion plasma and suggests improvements in the mainline tokamak and mirror concepts.

The good technical progress that motivates our support of this program can be summarized in large measure on the quality of confinement and temperature chart (Figure 2). The results of tokamak and mirror experiments over the previous decade are shown by broad areas of experimental exploration. The ratio of energy produced in these experiments, if they had used deuterium and tritium fuel, to the required heating energy, the "equivalent gain", has increased from less than 10^{-8} to more than 10^{-2}, a factor of more than one million. Today, we are embarked on an experimental campaign to demonstrate the scientific feasibility of fusion in a deuterium-tritium plasma with the TFTR device by 1986. We will also attempt to produce a reactor-grade plasma in the MFTF-B device by 1988. Shortly thereafter, we should be in a position to specify the plasma core most appropriate for reactor development, perhaps as the core of a Fusion Engineering Development device.

In spite of the appearance of smooth progress in Figure 2, the task of managing the magnetic fusion program is far from smooth. Basically, this is

Increasing Achievements and Facility-Based Expectations Underly the Program Strategy

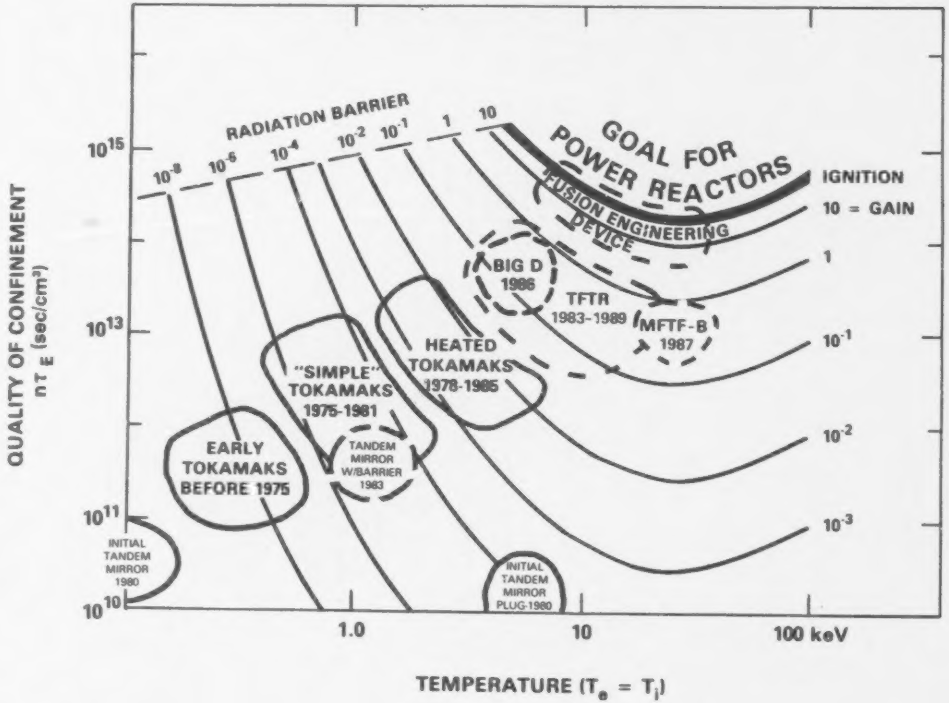


Figure 2

because of the scientific and technological complexities of the program. These technical complexities determine the special needs of fusion scientific development that underlie the program management plans and actions reflected in the proposed budgets.

After the demonstration of scientific feasibility, a great deal of both scientific and technological research remains before practical application. Our proposed budget represents both the effort required to demonstrate scientific feasibility and to prepare the research program for this practical application.

A close examination of the technical situation indicates that the physics and technology issues can be treated differently in planning this research program. In the physics area, all the physics issues, shown as a set of eight on Figure 3, must be considered as a closely integrated system. This follows from the fact that investigation of any one problem quickly leads to involvement of all in an interactive way. This means that research on the physics issues must be conducted in completely integrated facilities. Much of our budget is based on the need to build and operate them. These facilities are, themselves, sometimes referred to as experiments because of the natural integration of all the physical phenomena. However, each of our facilities emphasizes a particular set of physics experiments. We strive for the minimum overlap to definitively resolve the scientific issues.

One of the important fixed points in the future planning which underlies our budget proposal is effective utilization of this set of major experimental facilities which represent the past capital investment in the program.

THE EIGHT FUSION PHYSICS ISSUES MUST BE ADDRESSED AS A COMPLETE PLASMA SYSTEM

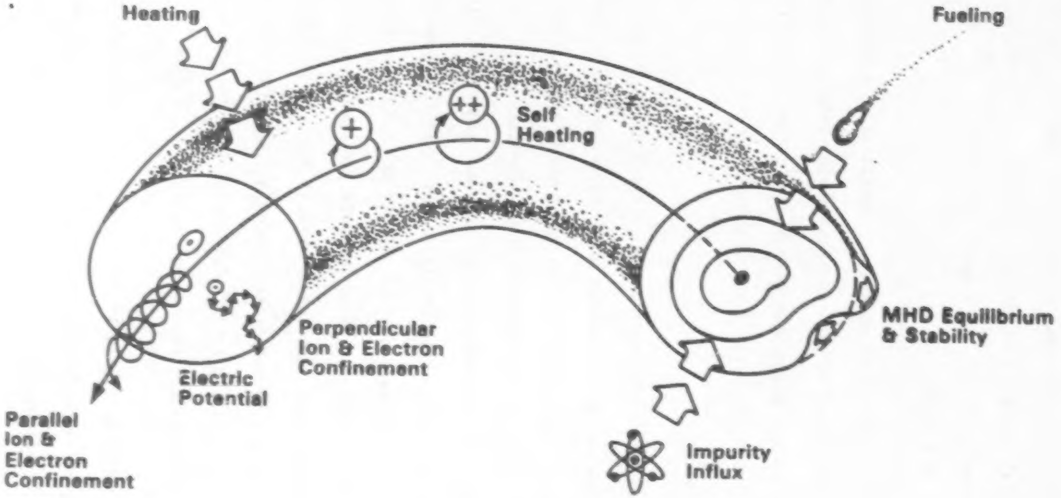

Heating
Fueling
Self Heating
MHD Equilibrium & Stability
Perpendicular Ion & Electron Confinement
Electric Potential
Parallel Ion & Electron Confinement
Impurity Influx

Figure 3

Figure 4 indicates the overall schedule for utilization of the three central program facilities for integrated plasma system testing. Use of these existing facilities along with their possible upgrades is essential to conducting the program in a cost-effective manner. As shown, the research programs on all three facilities are intended to support the reactor core selection process in the late 1980's. These facilities constitute an almost complete set of required experiments. We are presently defining a facility to take the next major scientific step in the program, plasma ignition and equilibrium burn.

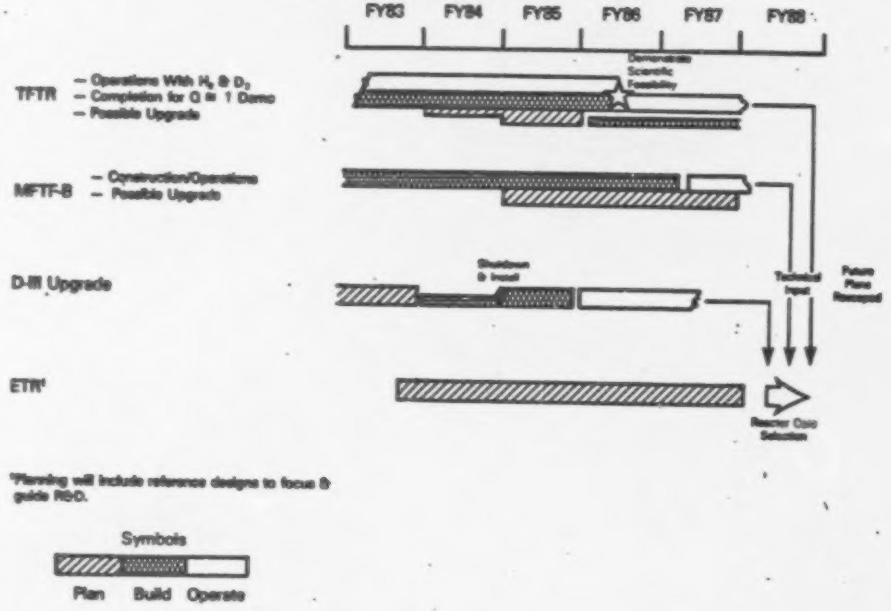
Unlike physics issues, technology research issues can be resolved largely through development programs in each individual technology. Ultimate tests of system integration will still be required in a complete fusion system before practical application, but that is clearly the final step in this technolgoy development. A facility to accomplish this technological integration, called the Engineering Test Reactor (ETR), is under design as indicated in Figure 4. Figure 5 indicates in a convenient classification, those individual technology activities that are being pursued within the program budget. The lower portion of the figure points out the existing or planned facilities in which these studies could be done. Our program uses engineering systems design studies to ensure that all necessary technologies are being considered and that the priority of development is appropriate.

International Cooperative Activities

As noted in the previous discussion of program management, we are using international cooperation wherever it can be seen to be for the mutual

The Existing Major Experimental Facilities And The ETR Are The Central Elements Of The Implementation Process

Construction/Operation Sequence



*Planning will include reference designs to focus & guide R&D.

Figure 4

THE THREE CLASSES OF FUSION TECHNOLOGIES CAN BE DEVELOPED INDIVIDUALLY TO A HIGH DEGREE, BUT MUST ULTIMATELY BE TESTED TOGETHER IN AN INTEGRATED SYSTEM

	PLASMA PRODUCTION TECHNOLOGIES	NUCLEAR TECHNOLOGIES	SYSTEM TECHNOLOGIES
PROGRAM AREAS	• MAGNETS • CRYOGENICS • R.F. SYSTEMS • NEUTRAL BEAM SYSTEMS • PLASMA FUELING • POWER SUPPLIES & ENERGY STORAGE • PLASMA DIRECT RECOVERY • VACUUM	• FIRST WALL COMPONENTS • BLANKETS • FUEL HANDLING • SHIELDING	• INSTRUMENTATION AND CONTROL • SAFETY & ENVIRONMENTAL • MAINTENANCE & REMOTE HANDLING
PROGRAM FACILITIES	BASE PROGRAM – FACILITIES FOR FEASIBILITY STUDIES		
	• CURRENT SCIENCE & TECHNOLOGY FACILITIES	• TFTR, TSTA FISSION REACTORS	• TFTR, MFTF
	REACTOR ENGINEERING PROGRAM – FACILITIES FOR ENGINEERING DEVELOPMENT		
		• ETR & ANCILLARY TESTS	• ETR & ANCILLARY TESTS

Figure 5

advantage of all parties to the collaborations. This includes construction of unique, large-scale facilities. The list of current cooperative activities is shown in Figure 6.

In particular, international cooperation would be used during the 1980's to complement the U.S. fusion program, to maintain its breadth, particularly in the area of alternate concepts and, where feasible, to reduce program costs by sharing the expense of building and operating selected facilties. For example, stellarator concept development is being done primarily in Europe, Japan, and the USSR, while only enough work to exploit new discoveries is done now in the United States. Also, reversed field pinch experiments carried out in Europe and bumpy torus experiments in Japan complement those done in the United States. The Large Coil Test Facility is being constructed in the United States, but half of the coil complement is being provided by the European Community, Switzerland, and Japan.

Although the United States program is largely self-sufficient in tokamak and mirror development, it would benefit from international cooperation in these areas as well. In particular, major foreign tokamaks are planned to be brought into operation in the mid- and late-1980's, and their results should help resolve specific tokamak issues not being fully explored here. International design studies, such as the INTOR Workshop series, have already aided in clarifying the program's understanding of key design issues and are expected to continue to do so.

INTERNATIONAL COOPERATION IS BEING INCREASINGLY USED TO MAINTAIN AND EXTEND PROGRAM BREADTH

SCIENCE	TECHNOLOGY	SYSTEM INTEGRATION
● STELLARATORS (US-EC) (US-J) (US-USSR)	● SUPERCONDUCTING COILS (IEA)	▲ INTOR (IAEA)
▲ BUMPY TORUS (US-J)	● PLASMA-WALL INTERACTION (IEA)	▲ LARGE TOKAMAK DEVICES (IEA)
▲ THEORY INSTITUTE (US-J)	● MATERIALS IRRADIATIONS (US-J) (IEA) (ESPECIALLY FMIT)	○ ETR/FER (US-J)
▲ REVERSED FIELD PINCH (US-EC)	○ REMOTE MAINTENANCE AND TRITIUM HANDLING (US-CANADA)	
▲ MIRROR (US-J) (US-USSR)	○ NUCLEAR TOPICS (US-J, US-FRG) (BLANKET, SHIELD, ETC.)	
▲ TOKAMAK (US-J, US-EC, US-USSR)	○ DIVERTORS (US-FRG)	

● - COOPERATION ESSENTIAL TO US PROGRAM SUCCESS IN THIS AREA

▲ - COOPERATION HELPFUL TO US PROGRAM SUCCESS

○ - COOPERATION BEING SERIOUSLY CONSIDERED

Figure 6

CONFINEMENT SYSTEMS (TOROIDAL)

With this general summary of the technical content of the program, let me turn to a more specific description of our technical progress this past year. Let me begin with the progress in the Toroidal Confinement Systems Division. The Toroidal Confinement Systems program consists of our major tokamak and tokamak/stellarator experimental programs. The tokamak is our most mature confinement concept and, in recent years, in different devices, has achieved near-reactor conditions in quality of confinement and in temperature. We now believe that we could design a tokamak-based pulsed fusion reactor. And in the Tokamak Fusion Test Reactor (TFTR), shown in Figure 7, we expect to achieve reactor-level values of both confinement and temperature to confirm this design.

The tokamak itself is by far the largest part of the toroidal program and efforts are underway to extend the data base for the conventional tokamak and to improve upon the tokamak concept by making it capable of very long-pulse (quasi-steady-state), higher beta operation. The stellarator is a close relative of the tokamak that can lead to a better understanding of the physics of toroidal confinement systems and broaden the scientific base for anticipated tokamak improvement. Design work has, therefore, begun on a modest stellarator-type device with the objective of contributing to the understanding and optimization of the toroidal concept.

Good progress has been made this year on the eight fusion physics issues discussed earlier. In the Toroidal Confinement Systems subprogram, this progress is indicated in Figure 8. The most important results have been in

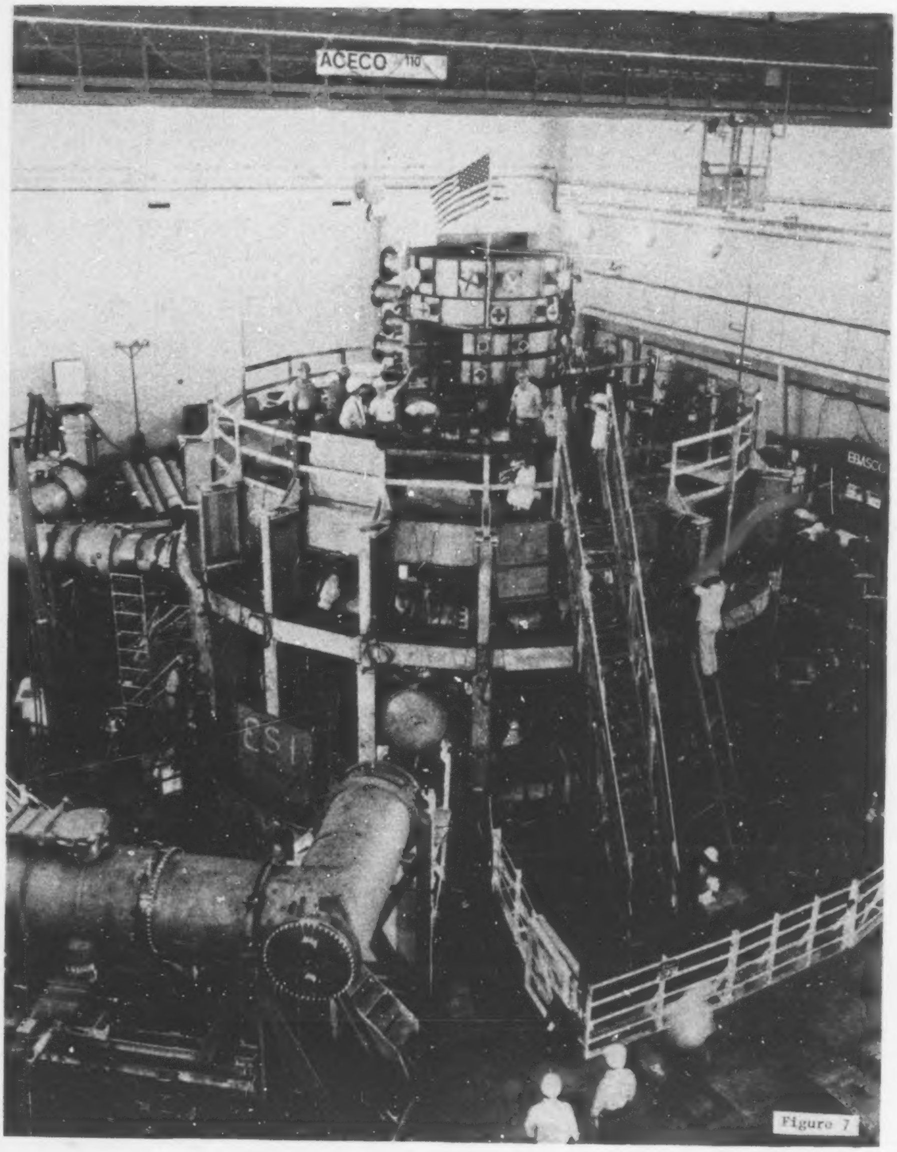

ACECO 110
BEMSCO

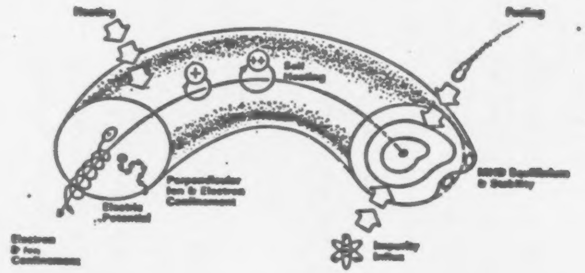
Figure 7

the area of equilibrium and stability. We have had initial successes in efforts to demonstrate that the tokamak can be operated with long enough pulse lengths and high enough plasma pressure to alleviate a number of the engineering problems of a reactor. In addition, we completed seven years of construction of the Tokamak Fusion Test Reactor at Princeton Plasma Physics Laboratory, with first plasma on Christmas Eve.

Because the tokamak configuration includes a plasma current driven by currents changing in a transformer, the configuration is one of finite pulse length. For some time, the pulsed nature of the tokamak configuration has been seen as a limitation of the reactor concept. More detailed work indicates that pulse lengths of about 10,000 seconds would be sufficient to alleviate cyclic fatigue problems in a reactor. In recent years, our theoreticians have begun to identify methods of driving the tokamak current with external sources of radio frequency or particle beam power. Now, we have our first experimental results, from Princeton Large Torus (PLT) and Alcator C at MIT, which indicate that, in principle, long pulse operation is certainly feasible and that true steady-state current drive is possible, although considerable improvement in efficiency will be required.

The specifics are that in PLT, modest RF power was used to drive the full current for a fraction of a second and to drive about half the current for several seconds. This was done at low plasma density in order to match the existing RF system which had originally been built for heating experiments. These experiments show that combining the tokamak transformer and RF power

PROGRESS ON THE EIGHT FUSION PHYSICS ISSUES IN TOROIDAL SYSTEMS


Heating
Fueling
Self Heating
MHD Equilibrium & Stability
Perpendicular Ion & Electron Confinement
Electric Potential
Electron & Ion Confinement
Impurity Influx

- o EQUILIBRIUM & STABILITY
  - -- BETA 4.7% ON DOUBLET III
  - -- POSSIBILITY OF VERY HIGH BETA WITH AIF CONFIGURATION
  - -- RF CURRENT DRIVE ON PLT (0.4MA) AND ALCATOR C (0.2MA)
- o HEATING
  - -- RF HEATING ON PLT (3MW, ICRH) ALCATOR C (250KW, LH) AND DOUBLET III (400KW, ECH)
- o FUELING
  - -- MULTI-PELLET RESULTS ON PDX, ALCATOR C
- o PERPENDICULAR TRANSPORT
  - -- GOOD CONFINEMENT WITH 3.5MW BEAMS ON DOUBLET III AND PDX
- o IMPURITY INFLUX
  - -- PUMPED LIMITERS INSTALLED ON ISX, PDX
- o SELF HEATING
  - -- TFTR FIRST PLASMA
- o ELECTRIC POTENTIAL
  - -- DIAGNOSTIC INSTALLED ON ISX
- o PARALLEL TRANSPORT
  - -- NOT A TOROIDAL ISSUE

Figure 8

to drive a quasi-steady-state current of long enough duration to avoid engineering problems of pulsed operation would be practical. On Alcator C, a higher frequency RF system, also built originally for heating experiments, has been used to drive current but in this case at higher plasma densities, closer to those proposed for reactor operation. This indicates the possibility of true steady-state current drive, although the economic optimization has yet to be done. We are continuing to work to find ways of increasing the efficiency of steady-state current drive using this and other methods. Design of a small, high field, long pulse tokamak, the Alcator DCT, is under study at MIT to extend our capability to study quasi-steady-state tokamak issues.

The issue of equilibrium and stability has also yielded to another concerted effort this year. A key element of this issue is the limit on plasma pressure that can be contained by a given magnetic field. We use the symbol beta (β) to indicate this ratio of plasma pressure to magnetic field and use this as an indicator of the efficiency of the magnetic field in a fusion device. Beta is, therefore, an important indicator of economic potential. For the tokamak configuration, a beta of 5-7% is thought to make an adequate reactor, although a higher value would be desirable for a truly attractive reactor. This year in Doublet III at GA Technologies, Inc., we achieved a beta of 4.7% utilizing specially shaped plasmas (dee-shaped cross-section instead of the standard circular) and neutral beams as heaters to increase the plasma pressure. This is very near the minimum required, and we expect to go beyond 5% when more heating power is applied.

At the same time, extensive computational work has been carried out at ORNL and other U.S. institutions on an advanced toroidal configuration to explore the physics of high beta, steady state toroidal confinement. The advanced toroidal configuration identified for experimental test at ORNL is a type of stellarator and appears to be capable of operating in a regime of high beta known as the second stability regime. This is a regime of great importance to toroidal systems such as the tokamak, and experimental tests of computer codes on the ATF-1 will be directly relevant to the design of larger, high beta tokamak devices. In addition, the systematic varying of parameters such as plasma current density and helical magnetic field possible on such a device, combined with data from tokamak devices, will provide a much more complete picture of the physics underlying tokamak confinement.

The evolution of toroidal fusion ideas is shown in Figure 9. The simple torus has no end losses but is not a stable configuration. Stability requires a component of magnetic field going the short way around the torus. In the tokamak, this field is produced by current in the plasma; in the stellarator, helical coils (shown as modular helical coils) provide this field. The ultimate, optimized toroidal confinement concept may well be a combination of these two, with any internal plasma current driven by external sources, such as RF. In the meantime, the close relationship between the tokamak and the stellarator results in a strong and rapid transfer of understanding from one to the other.

EVOLUTION OF TOROIDAL FUSION IDEAS

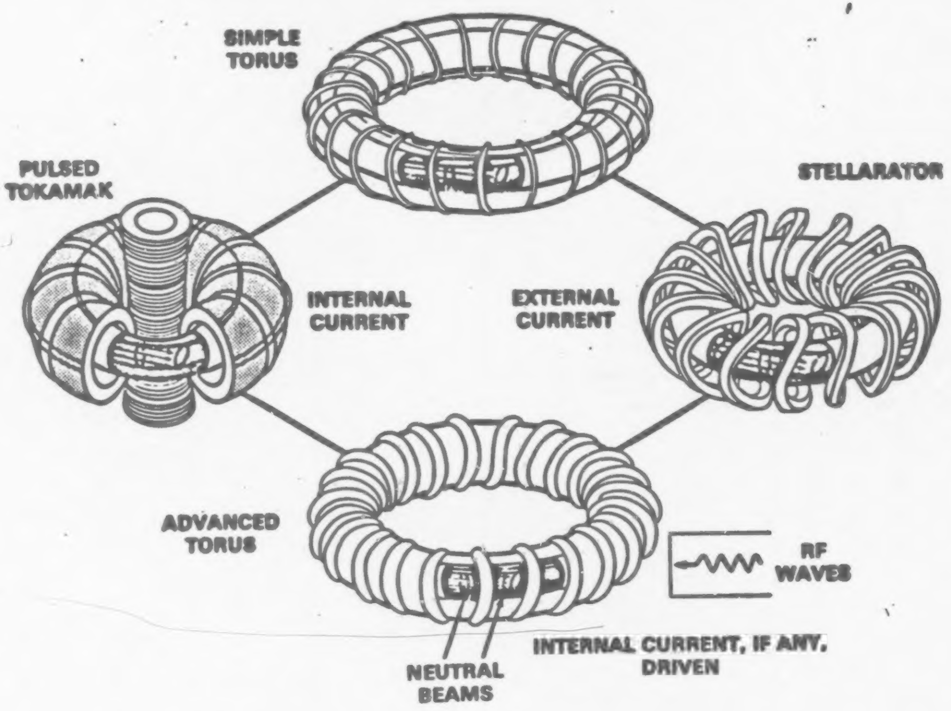

SIMPLE TORUS
PULSED TOKAMAK
STELLARATOR
INTERNAL CURRENT
EXTERNAL CURRENT
ADVANCED TORUS
RF WAVES
INTERNAL CURRENT, IF ANY, DRIVEN
NEUTRAL BEAMS

Figure 9

We are, therefore, proceeding to convert the ISX facility at ORNL into the ATF-1, at a cost of about $20M, utilizing the existing site, power supplies, diagnostics, and heating systems. The present ISX team will operate ATF-1 beginning in late 1986.

Returning to the tokamak itself, perpendicular transport is, in tokamaks, the basic issue of good energy confinement. The tokamak has been shown to have very good confinement properties, but we had observed a deterioration of the best confinement with the addition of high power neutral beam heaters. Now, recent experiments showing continued good confinement during powerful heating on ASDEX in W. Germany have been repeated and extended on Doublet III and Poloidal Divertor Experiment (PDX). A new operational mode produced by a subtle change in the magnetic field configuration leads to good confinement with up to 3.5MW of beam power. These experiments are continuing now at increasing power levels.

There has also been progress on other physics issues: RF heating on PLT at the ion cyclotron frequency is proceeding well; lower hybrid frequency heating looks promising on Alcator C; and electron cyclotron heating looks encouraging on DIII. Each of these methods has different reactor advantages and is being pursued in a search for the optimum heating method. In fueling, multi-pellet injectors have been used to maintain and shape the plasma density in PDX and Alcator C. In impurity influx, a critical issue for any long-pulse or steady-state confinement concept, poloidal divertor experiments continue successfully on PDX and improved designs for reactor versions have been developed. Pumped limiters, which may be simpler than divertors, have been installed on PDX and ISX. In the area of electric potential, a diagnostic has been installed on ISX to measure potentials and study their effects on confinement. Parallel transport is not considered to be an issue for toroidal systems, where there are no ends. A summary chart, or matrix, of the issues and devices in the toroidal program is shown in Figure 10.

The seven fusion physics issues relevant to toroidal confinement are shown vertically. On this chart, current drive is combined with heating since similar hardware (neutral beams or RF) is used. The specific physics issues are divided between the issues (or requirements) for an advanced engineering device such as an Engineering Test Reactor (ETR) and the issues for an optimized commercial reactor (Advanced Toroidal Issue). Horizontally listed are the Toroidal Confinement Systems major devices. The chart indicates that D-III Dee will replace D-III and ATF-1 will replace ISX-B. In FY 1984, only PLT, D-III, ISX-B (part of the year), Alcator C, and TFTR will be in operation. The matrix indicates how the program on each device contributes to the physics issues in the toroidal program.

In addition to physics progress, considerable progress has been made in fabricating hardware for future toroidal experiments. During FY 1982, most of the Tokamak Fusion Test Reactor (TFTR) construction project effort was directed toward assembly of the TFTR tokamak. The vacuum vessel, coils, and support structures were assembled into one structure as seen in Figure 7 above, and subsystem testing was started immediately thereafter. Upon

Toroidal Program Matrix

MAR 8, 1983

			PLT	ISX-B ——→	ATF	D-III ——→	D-III DEE	ALC.C	TFTR
Status			Operating	Operating	Conceptual Design	Operating	Engineering Design	Operating	Integrated Testing
Unique Capability			ICRF, Current Drive	High β	Toroidal Optimization	Low q Shaped Plasmas	ETR Plasma Engineering	High Field High Density	Reactor DT Plasmas
Physics Issues	Tokamak ETR Issues	Advanced Toroidal Issues							
Heating & Current Drive	ICRF Non OH Start-Up		2.0 MW ICRF, LH Current Drive Start-Up				ETR Prototype		
		Steady State, Best Heating Method			Steady State Demonstration	ECH (High Power)		LH Current Drive at High N	
Fueling	Pellet Fueling							Four Pellet Injector	
		Same as for ETR							
Equilibrium & Stability	β = 5%, Disruption Control			2.5% β, Circular Shape		Low q Shape Optimization β = 4.7%	ETR Prototype		
		Higher β Values			High β, Disruption Free				
Perpendicular Transport	Minimum Size for Q~5		Transport at High Temp.					Transport at High Density	Major Size, Temp. Scaling
		Minimum Size for Ignition			Three Dim Confinement				
Impurity Influx	Mechanical Vs. Magnetic Diverter			Pumped Limiter		Expanded Boundary	ETR Prototype		
		Optimized Control Method							
Self Heating	α Particle Testing		Simulation With Beams						Study α Particle Deposition
		Same as for ETR							
Electric Potential	Thought to Have Some Effect on Transport			Exp. to Meas. El. Pot.					
		Same as for ETR							

Figure 10

completion of the subsystem testing, an integrated system test was performed in December 1982. This was the "first plasma test" which was successfully completed early in the morning on Christmas Eve. This was truly an exciting event which provided an initial validation of the design and fabrication efforts of hundreds of dedicated individuals. It also provided optimism for the experimental research program to be conducted over at least the next four years in TFTR.

Other TFTR program accomplishments in FY 1982 included successful testing of neutral beam power equipment at PPPL, fully satisfactory progress in preparing the diagnostic instruments which will be used to observe the plasma, and completion of design solutions for the high power armor plating which will be added to TFTR as part of the ongoing TFTR Flexibility Modifications.

At GA Technologies, Inc., (GA), the fabrication of a high power, multi-second electron cyclotron heating system continues; experiments will begin next year after installation of the couplers on Doublet III. This will be the major test of ECH for heating, current drive, and configuration control on tokamaks. Also at GA, the design effort for a new dee-shaped vacuum vessel is well underway. With this new vessel, Doublet III will, in 1986, become the major U.S. hydrogen device for addressing tokamak ETR issues, both physics and technology. The present and the modified device are depicted in Figures 11 and 12.

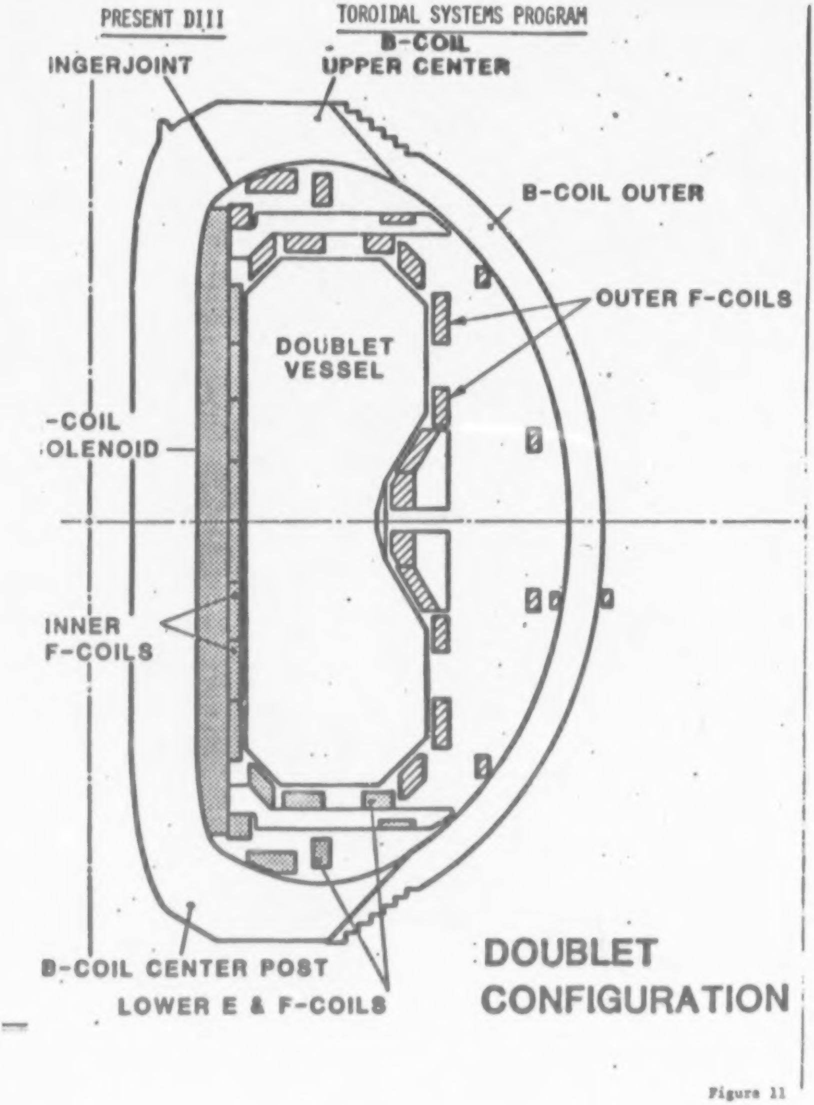

PRESENT DIII
TOROIDAL SYSTEMS PROGRAM
B-COIL
UPPER CENTER
INGERJOINT
B-COIL OUTER
OUTER F-COILS
DOUBLET
VESSEL
-COIL
OLENOID
INNER
F-COILS
B-COIL CENTER POST
LOWER E & F-COILS
DOUBLET
CONFIGURATION

Figure 11

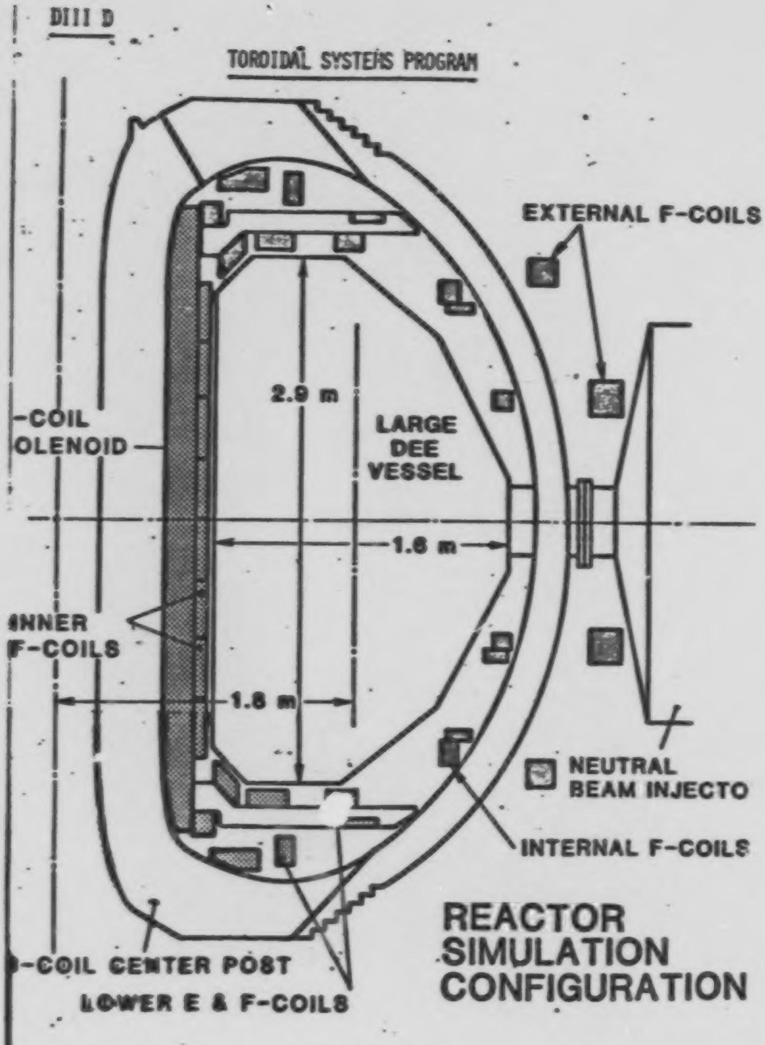

DIII D
TOROIDAL SYSTEMS PROGRAM
EXTERNAL F-COILS
2.9 m
LARGE DEE VESSEL
-COIL
OLENOID
1.6 m
INNER F-COILS
1.8 m
NEUTRAL BEAM INJECTO
INTERNAL F-COILS
-COIL CENTER POST
LOWER E & F-COILS
REACTOR SIMULATION CONFIGURATION

Figure 12

TOROIDAL PROGRAM STATUS & GOALS

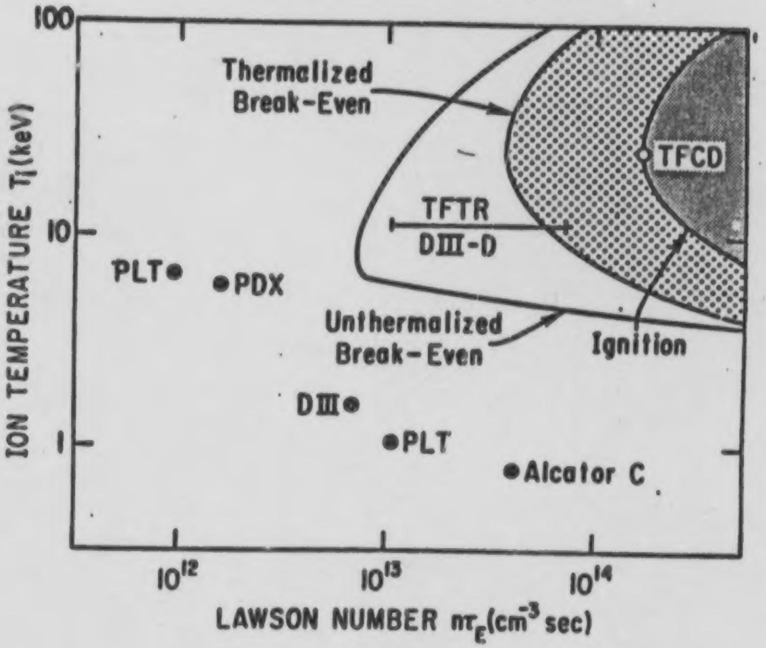


Figure 13

The status and the technical objectives of the toroidal program are summarized in the plot of ion temperature versus Lawson number (or quality of confinement) in Figure 13. The tokamak is the most mature fusion concept and is moving toward the energy break-even curves with TFTR and DIII. The first curve, "unthermalized break-even" refers to a beam/plasma device where much of the fusion reactions come from interaction of the beam with the plasma. "Thermalized break-even" occurs when plasma interactions alone produce as much energy as is required to heat the plasma. Ignition occurs when the reaction products continue to heat the plasma and external heaters can be turned off. TFTR, which will operate with the reactor fuels, deuterium and tritium, and Doublet III, without tritium but with a vacuum chamber shaped more like a reactor, should both operate in this physics regime.

The Tokamak Fusion Core Demonstration (TFCD) is a new element in our planning. It has grown out of consideration of an upgrade to the TFTR facility and is seen as testing the physics of ignition and equilibrium burn, that is, of a reactor core. The Magnetic Fusion Advisory Committee has indicated that the study of the equilibrium burn of a fusion plasma is the next major objective of the fusion program. The TFCD is seen as the appropriate next step in the tokamak program, prior to an advanced engineering device, shown in our plans as the Engineering Test Reactor, ETR. A plan for the development of the toroidal concept, incorporating these elements is shown in Figure 14. Included are references to foreign tokamak programs and upgrades to facilities which are in varying stages of development and consideration.

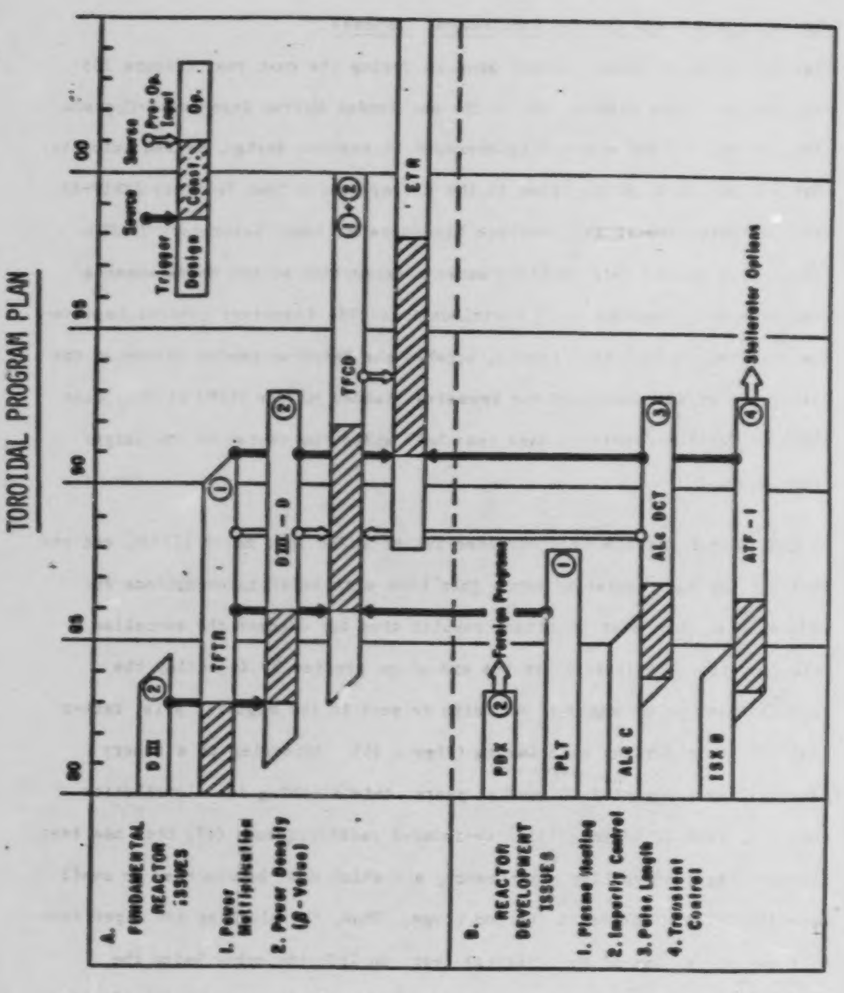

TOROIDAL PROGRAM PLAN
80
85
90
95
00
A. FUNDAMENTAL REACTOR ISSUES
1. Power Multiplication
2. Power Density (β-Value)
B. REACTOR DEVELOPMENT ISSUES
1. Plasma Heating
2. Impurity Control
3. Pulse Length
4. Transient Control
D III
TFTR
D III – D
TFCD
ETR
PDX
PLT
ALc C
ALc DCT
ISX B
ATF – I
Foreign Program
Stellarator Options
①
②
① + ②
③
④
Trigger
Source
Source
Pre-Op. Input
Design
Const.
Op.

Figure 14

Confinement Systems (Mirror Confinement Systems)

Highlights in the tandem mirror program during the past year (Figure 15) included the first experiments on the new Tandem Mirror Experiment-Upgrade (TMX-U) facility and a major improvement in reactor design, called axicells, that has now been incorporated in the Mirror Fusion Test Facility (MFTF-B) under construction at the Lawrence Livermore National Laboratory (LLNL). Design work in the Tara facility under construction at the Massachusetts Institute of Technology (MIT) contributed to this important concept improvement. Other smaller experiments, notably the Phaedrus tandem mirror at the University of Wisconsin and the Symmetric Tandem Mirror (STM) at TRW, continue to provide important data that help shape the course of the larger programs.

At LLNL, first plasmas were obtained in the TMX-U last March (1982), and the facility has been operating since that time with brief interruptions for maintenance. The most important results thus far concern the so-called "sloshing ion distribution" in the end plugs created by injecting the neutral beams at an angle of 47° with respect to the magnetic axis, rather than 90° as in earlier experiments (Figure 16). According to a theory developed over a period of several years, this sloshing ion distribution should be free of microinstability-induced radiofrequency (rf) that has been characteristic of earlier experiments, and which must be overcome to avoid excessive plasma losses in the end plugs. Thus, the sloshing ion experiment is regarded as one of two critical tests in TMX; the other being the creation of thermal barriers by electron cyclotron heating (ECH). Sloshing ions were first established in the TMX-U end plugs beginning in May 1982,

(Figure 15)

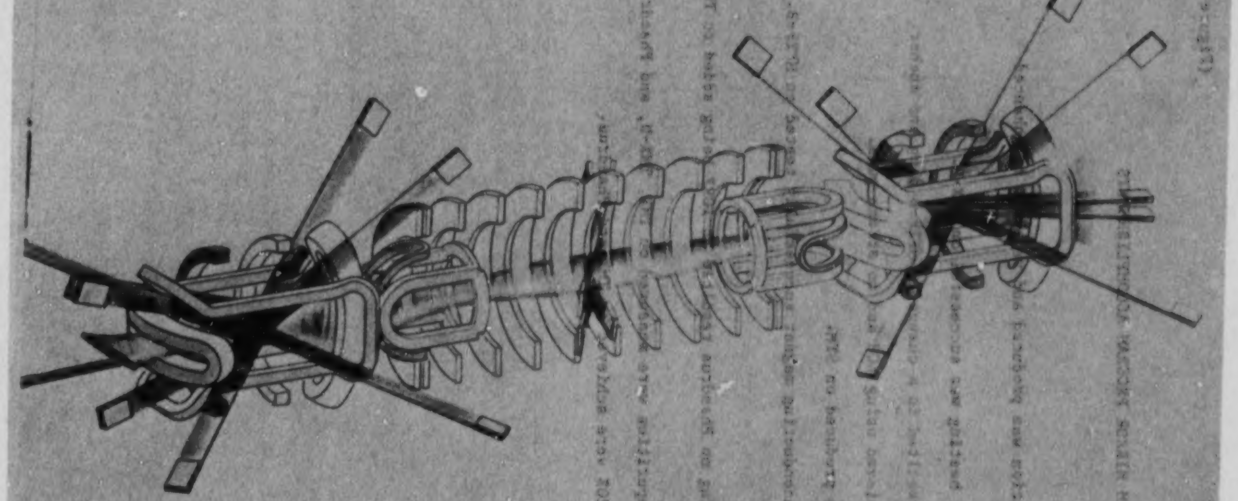
TANDEM MIRROR PROGRAM ACCOMPLISHMENTS

End Plug and Anchor

- Sloshing ion distribution was produced and resulted in enhanced microstability.
- Electron Cyclotron rf heating was successful on TMX-U.
- Tara axicell design resulted in a change in MFTF-B plug and anchor.
- TMX-U startup was achieved using ECH and no stream guns.
- Disk of hot electrons produced on STM.
- World's largets supreconducting magnet successfully tested on MFTF-B.

Center Cell

- Successful ICRF heating on Phaedrus resulted in ICRF being added to TMX-U and MFTF-B.
- Very low levels of impurities were measured on TMX, TMX-U, and Phaedrus.
- Average betas up to 20% were achieved on TMX and Phaedrus.



TMX

Lawrence Livermore National Laboratory

Magnet coils, plasma contour, and energetic neutral beams of TMX-U

Figure 16

and indeed it was found that the rf level was a factor of ten less than that observed earlier in TMX, consistent with theoretical expectations. This is a major accomplishment for the tandem mirror program that greatly increases confidence in scaling to MFTF-B and reactors.

ECH experiments have also begun on TMX-U. In preliminary experiments with as much as 60-80 kilowatts injected into each plug, there is evidence of strong heating of the electrons as measured by x-ray detectors. As a bonus, it was found that startup of the plasma could be initiated by ECH and neutral beams alone, thereby simplifying the startup process on MFTF-B and later devices.

In other experiments, investigation of ion cyclotron resonance frequency (ICRF) continues in the Phaedrus device at Wisconsin. Earlier Phaedrus results have led to the introduction of ICRF as an additional heating source for tandem mirrors, including TMX-U and MFTF-B. These results, when combined with the earlier results from TMX—namely, demonstration of electrostatic potential confinement, up to 20% average beta values in the center cell, and very low levels of impurities—are very encouraging for the tandem mirror program. On the STM device at TRW, disks of hot electrons have been formed. The understanding of the mechanism for producing these disks is important for producing an effective thermal barrier on tandem mirrors.

A conceptual tandem mirror advance, called the axicell approach, has resulted from work on conceptual reactors. The axicell is a high-field circular coil set at each end of the center cell that permits a much higher plasma density and therefore higher fusion power production in the center cell. A related idea is incorporated in the Tara facility at MIT, which will begin operation in FY 1984. During the past year, this improved design approach was also incorporated in the MFTF-B (Figure 17).

As this example shows, the MFTF-B, which is the major scaling experiment for the tandem mirror program, has been designed as a very flexible facility that can take advantage of the many improvements being discovered on present facilities. A year ago, the MFTF-B construction project passed the half-way mark with a successful test of one of the world's largest superconducting magnets. The MFTF-B will begin operation in FY 1986, and will provide a definitive test for scaling the tandem mirror to the reactor regime.

At a recent meeting with Japanese and Soviet scientists at Livermore, California, the critical physics issues of the tandem mirror program were reviewed and a planning matrix was developed to assure that there would be research on all the key issues, utilizing both present tandem mirror facilities and those under construction. U.S. mirror facilities are summarized in the program matrix of Figure 18. In Tsukuba, Japan, the Gamma 10 tandem mirror, about the size of TMX-U, has been commissioned, and neutral beams and ECH are being installed this spring. Ambal is a smaller tandem mirror which is beginning operation in Novosibirsk, USSR.

The Elmo Bumpy Torus concept, which is a closed toroidal configuration of magnetic mirrors, is another activity supported in the Mirror Confinement Systems program. The EBT program includes the EBT-S experiment at the Oak Ridge National Laboratory (ORNL), the EBT-P project, and several industrial

MFTF-B TANDEM MIRROR FACILITY

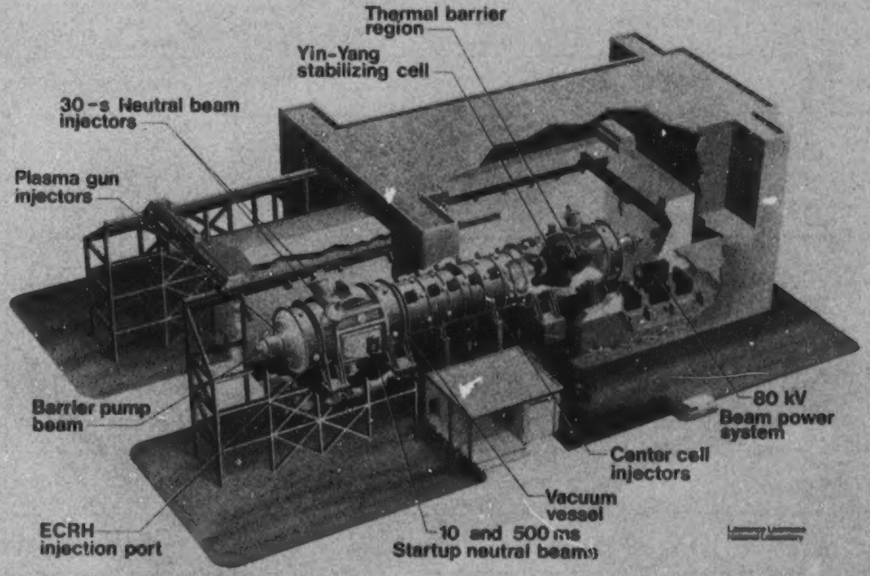

Thermal barrier region
Yin-Yang stabilizing cell
30-s Neutral beam injectors
Plasma gun injectors
Barrier pump beam
ECRH injection port
10 and 500 ms Startup neutral beams
Vacuum vessel
Center cell injectors
80 kV Beam power system

Figure 17

Tandem Mirror Program Matrix

3/83

		TMX-U	Phaedrus	STM	MMX	TARA	MFTF-B
Status		Operating	Operating	Operating	Operating	Under Construction	Under Construction
Unique Capability		Thermal Barriers	RF Test Cell	Electron Physics Test Cell	Independently Controlled Coils	Axisymmetry	Major Scaling
Physics Issue	Tandem Mirror Improvement Issue						
Heating	Steady State RF, ECRH, and Beams	Integrated Systems Tests	ICRH	ECRH Development			Long Pulse Tests
Fueling	To Use Pellets, Radial Diffusion, or Axial Trapping		RF Techniques				Radial and Axial Injection
Equilibrium & Stability	Microinstability Axisymmetry, and Beta Limits	Microstability in End Cells	Line-Tying Studies	MHD Stability by Rings	Stability-Boundary Tests	Hot Electron Anchors	Reactor Relevant Configuration; Beta Limits
Perpendicular Confinement	Not Uniform Along Length; Can Control Axial Profiles?	First Good Measurements in Tandem Mirror		Charge-Exchange Transport		Effects of Axisymmetry	Near Lawson Condition
Parallel Confinement	"Knob" to Adjust Particle Flows	Improved Regime for Mirrors			Multiple-Mirror Transport	Test Axicell	Near Lawson Condition
Impurity Control	Halo-Diverter Action	Gettering Techniques				Neutral Beam Design	Low-Impurity Beams; Cyropumps
Self-Heating	Alpha Particle Physics						Particle Simulations Planned
Electric Potential	Important to all Transport	Thermal Barrier Tests		Potential Barrier Studies			Radial, Axial Profile Studies

(Figure 18)

firms working on small physics contracts. During the past year, the EBT-S experimental program has made progress in ion heating, in developing split mirror coils to test a new method for injecting microwave power into the device, and in substantially improving the plasma diagnostics (Figure 19).

Two significant events have recently occurred which will influence the near-term technical and programmatic direction of the EBT program. First, the new diagnostics capability on EBT-S has led to substantial revisions in interpretations of the data, and increased uncertainties about the applicability of previously assumed scaling laws. Technical panels convened by the Department have recently reviewed the EBT program and concluded that highest priority in the coming year should be given to improving the accuracy and understanding of the EBT-S experiments, and in parallel, to improving the bumpy torus reactor concept by developing several new ideas which have been proposed.

With regard to the EBT-P project, the Department has decided to bring the present Title II design effort to an orderly conclusion and not to proceed with construction at this time. This action is consistent with the present technical and budgetary circumstances. The EBT-P design is based in many respects on physics models of the EBT plasma which were developed several years ago and are now called into question. It is technically prudent to assess the full implications of the recent EBT program reviews and results of planned near-term experimental and theoretical research before committing to construction of a next-step device in the EBT program.

(Figure 19)

BUMPY TORUS PROGRAM ACCOMPLISHMENTS

Physics Program

- Pulsed experiments and new diagnostics on EBT-S led to significant changes in interpretation of EBT plasmas.
- Control of T-M mode transition demonstrated.
- Improved ion and ring electron heating demonstrated on EBT-S.
- Several new ideas to improve plasma confinement and reactor power balance developed.

EBT-P Project

- Title I design completed, Title II design approximately 40% complete.
- Superconducting magnets and prototype limiters fabricated and successfully tested.

Development and Technology

Let me turn to the program supported in the Division of Development and Technology. The fusion technology program supports extensive research and development needed to develop the technologies involved in practical fusion energy systems. The basic fusion engineering task areas are shown in Figure 20. The program addresses the development of advanced components, the development and evaluation of fusion reactor systems and the materials associated with them, the conceptual design of commercial fusion reactors for both electric and non-electric applications, the design and development effort required to prepare for the selection of concepts for the initiation of an ETR project, and the evaluation and control of environmental and safety issues.

The technology program has developed components and subsystems for ongoing experiments and is now working on critical components and technologies for next generation devices. Shown on Figure 21 are the principal accomplishments in the last year-and-a-half in this program. Perhaps the best examples of work in the latter area are the development of large superconducting magnets for the Large Coil Test Facility and the development of tritium handling technology for the Tritium Systems Test Assembly.

The Tritium Systems Test Assembly (TSTA) shown in Figure 22 has been designed, constructed, and is now beginning operations at the Los Alamos National Laboratory. It will demonstrate the technology needed for fusion fuel processing and for associated personnel and environmental safety. TSTA is important both as a full-scale project in fusion engineering and as

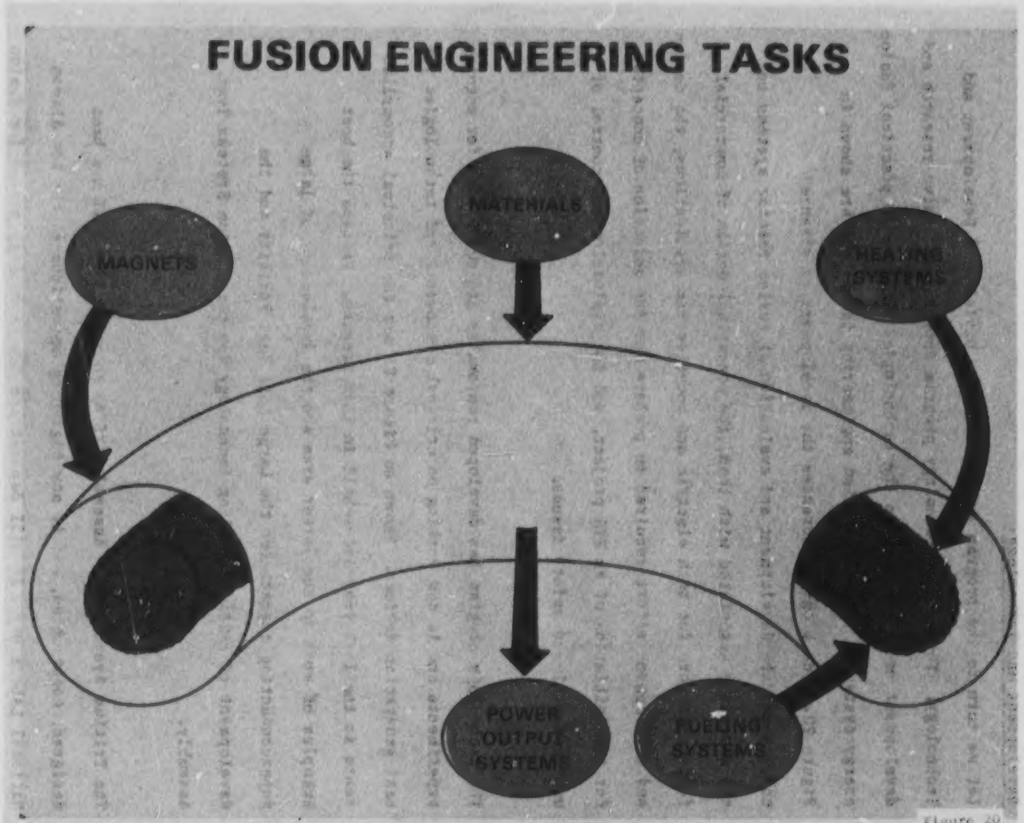

FUSION ENGINEERING TASKS
MAGNETS
MATERIALS
HEATING SYSTEMS
POWER OUTPUT SYSTEMS
FUELING SYSTEMS

Figure 20

DEVELOPMENT AND TECHNOLOGY ACCOMPLISHMENTS - FY82

PLASMA TECHNOLOGIES

- STARTED TESTING OF LARGE COIL TEST FACILITY AT ORNL
- JAPANESE LARGE COIL DELIVERED TO ORNL
- TESTED TFTR AND MFTF-B NEUTRAL BEAM SOURCES FOR SHORT PULSES AT NEW TEST FACILITY (NBETF) AT LBL
- 60 GHz GYROTRON INSTALLED IN PDX AND DOUBLET III

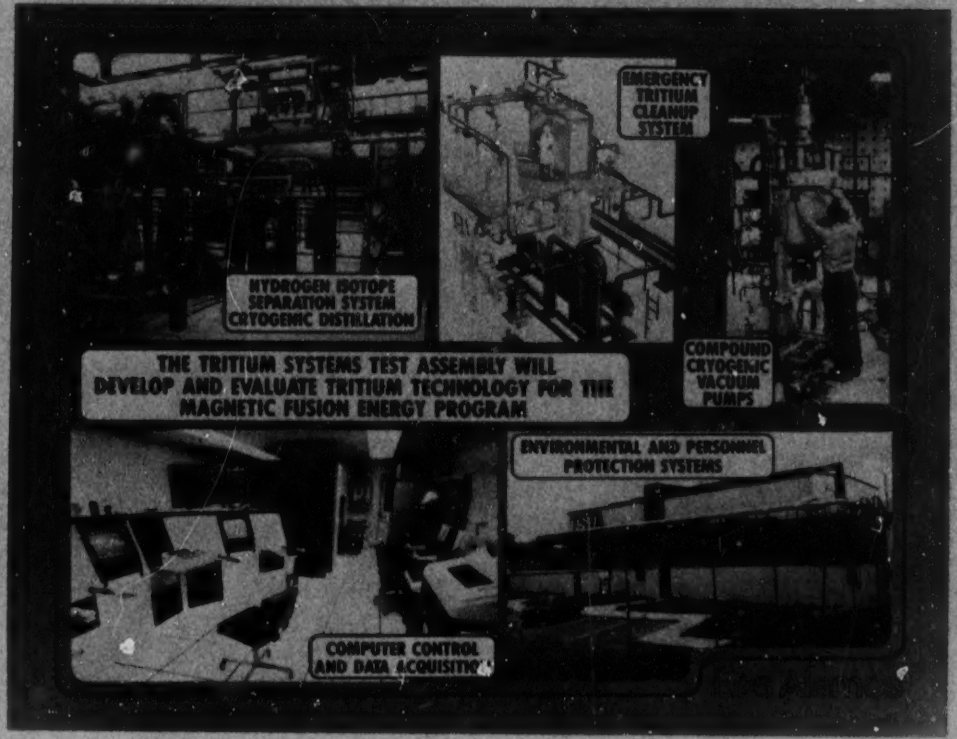
NUCLEAR TECHNOLOGIES

- COMPLETED CONSTRUCTION OF TSTA FOR DT FUEL CYCLE
- STARTED JOINT U.S.-JAPAN OPERATION OF RTNS-II AT LLNL
- COMPLETED TEST STAND FOR THERMOCHEMICAL TESTING OF FIRST WALL TEST SPECIMENS

REACTOR SYSTEMS

- COMPLETE DESIGN STUDY OF STEADY STATE TOKAMAK DEMO
- CONTINUE TANDEM MIRROR POWER REACTOR DESIGN (MARS)
- STARTED NUCLEAR SYSTEMS STUDY OF BLANKET/SHIELD DESIGNS

Figure 21


HYDROGEN ISOTOPE SEPARATION SYSTEM CRYOGENIC DISTILLATION
EMERGENCY TRITIUM CLEANUP SYSTEM
COMPOUND CRYOGENIC VACUUM PUMPS
ENVIRONMENTAL AND PERSONNEL PROTECTION SYSTEMS
COMPUTER CONTROL AND DATA ACQUISITION
TSTA
Los Alamos

THE TRITIUM SYSTEMS TEST ASSEMBLY WILL DEVELOP AND EVALUATE TRITIUM TECHNOLOGY FOR THE MAGNETIC FUSION ENERGY PROGRAM

Figure 21

a demonstration of our ability to handle tritium. To date, the necessary techniques have been designed by adapting from conventional chemical and mechanical processes, and the resultant systems have been built by private industry. Installation of equipment is now complete and initial checkout and testing are underway.

The Large Coil Test Facility (LCTF) under construction at Oak Ridge National Laboratory is entering the initial checkout phase. The facility was begun in 1977 as a part of an International Energy Agency agreement, wherein the United States was to provide a test facility to enable the testing of large superconducting magnet coils which are critical major system components that will be required to provide practical, economic magnetic fields to contain a fusion plasma. EURATOM, Japan, and Switzerland are each building a large superconducting coil and the United States is building three coils to meet common performance specifications and dimensions.

The LCTF was completed in November 1982, so that it could initially be operated with up to three coils. Figure 23 shows the Japanese coil, the first to be completed, being installed into the test facility. It is planned to check out all of the facility subsystems with one or two coils installed in the summer of 1983. In parallel, work will continue on completing the facility to accept all six coils by the end of 1983. The facility is unique to the world-wide international fusion effort, and will permit the rigorous testing that is needed to properly demonstrate that superconducting magnets needed for fusion will be available when needed.

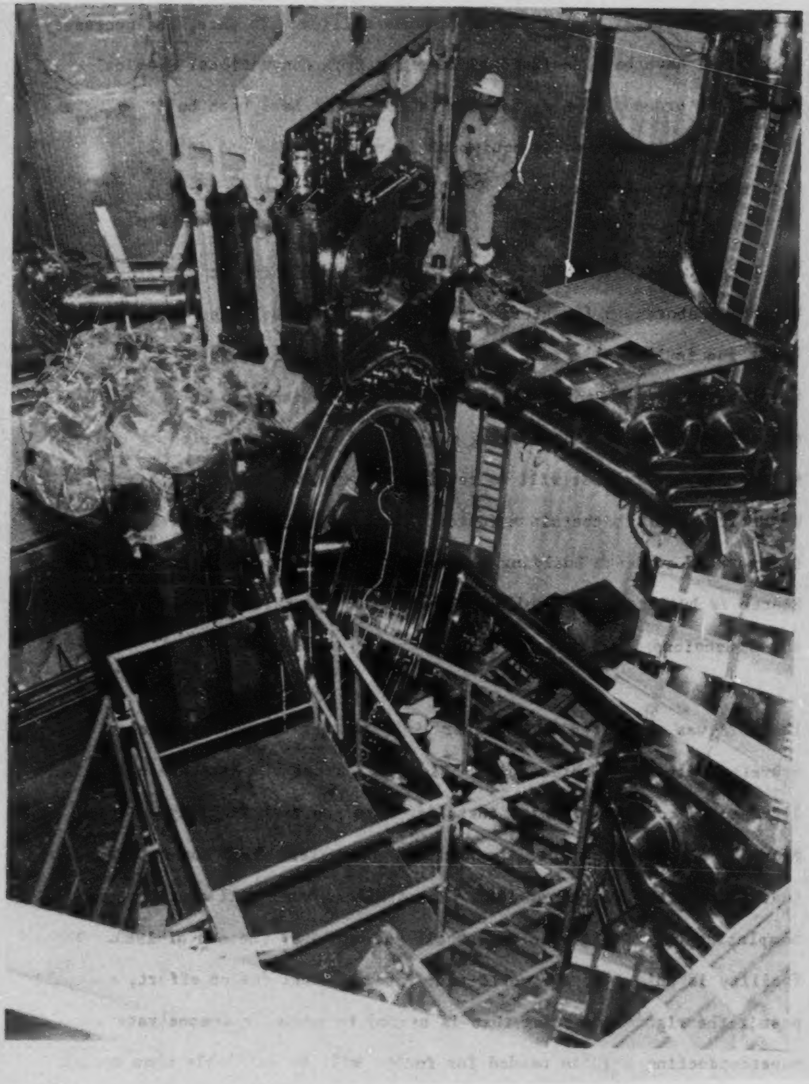


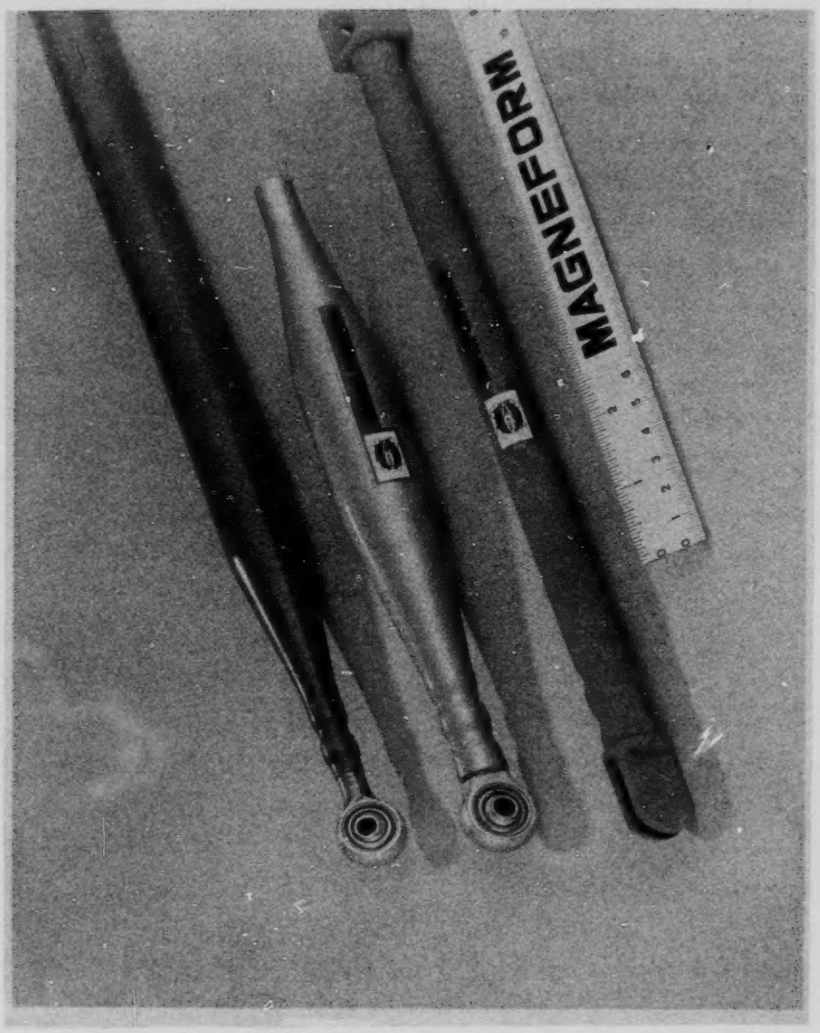
Figure 22

The Neutral Beam Engineering Test Facility (NBETF) at the Lawrence Berkeley Laboratory is providing a capability for long pulse tests of ion sources for neutral beam heating systems. It is an upgrade of the TFTR prototype neutral Beam Line and has already been used for short pulse testing of the MFTF-B and the TFTR positive ion sources. The facility is nearing completion with many systems and components now under final test. When completed in April 1983, the first task will be to thoroughly test and demonstrate the 80 KeV positive ion source MFTF-B technology for full 30 second pulses. Following that, the advanced source will be tested at 120 KeV for TFTR.

At the outset of this testimony, I pointed out that fusion is a goal oriented energy R&D program that has additional benefits from its high technology developments. In past years, we have identified the potential for technology spin-offs from the fusion program. This was thoroughly documented in a DOE report (DOE/ER-0132 entitled "Technology Spin-Offs from the Magnetic Fusion Energy Program") in May 1982. The document identified some 138 developments that had roots in fusion R&D. Some of these spin-offs have indeed become practical products of industrial concerns, and two of these are explained below.

Metal Forming - Magniform

It has been known for a long time that magnetic fields can exert strong forces on electrical conductors carrying electrical current. However, the usefulness of transient forces on electrical conductors in pulsed magnetic fields was not fully appreciated before the early days of the magnetic

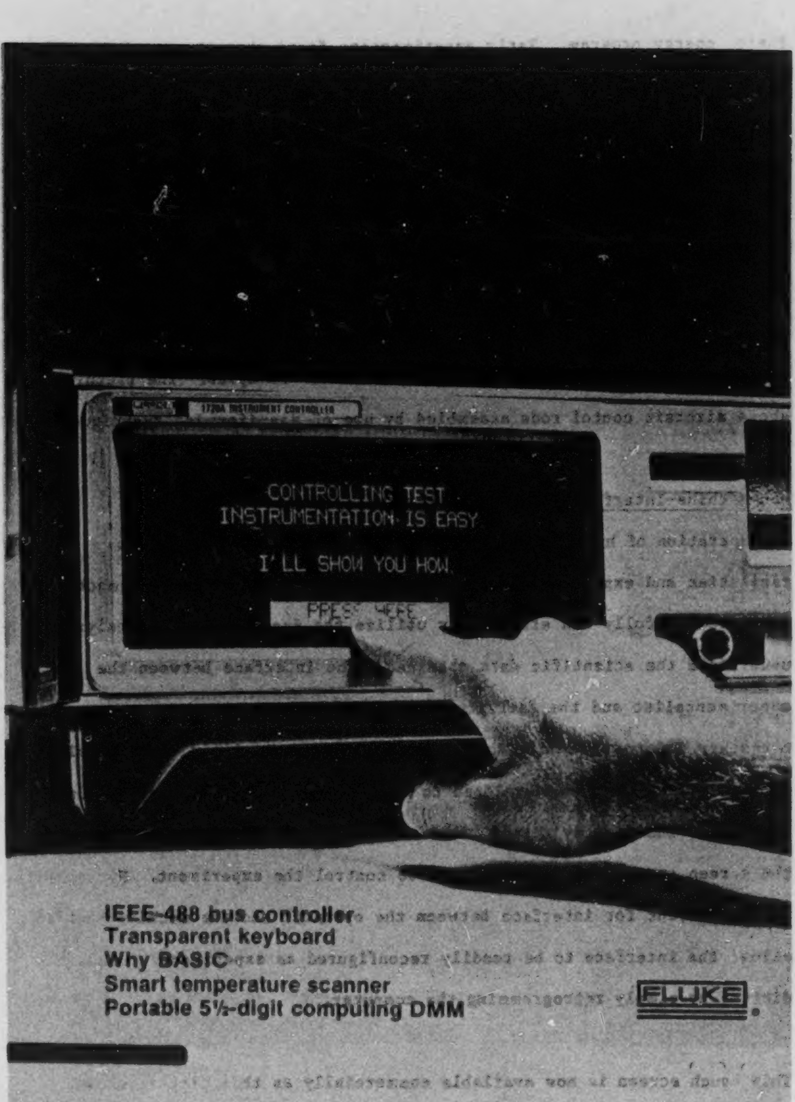

MAGNEFORM

fusion energy program. Early experimenters found that the metal liner of a fusion experiment could be collapsed by these transient forces. From this initial experience in the laboratory has grown a mature product line from Maxwell Laboratories in the forming of metal parts by pulsed magnetic fields. This technique has received wide application. Many diverse automotive parts such as fuel pumps, steering gear, oil filters, fan clutches and speed control diaphragms are assembled in this way. Electric motors, cigarette lighters, electrical insulators, aircraft control rods and breeder reactor fuel pin end closures are other examples. The picture shows aircraft contol rods assembled by use of magniforming techniques.

Man-Machine-Interface: CRT Touch Panel

The operation of highly complex, state-of-the-art magnetic fusion facilities and experiments requires careful attention to the man-machine interface to fully and efficiently utilize the facility and to analyze and understand the scientific data obtained. The interface between the experimentalist and the facility is critical. The computer can present any necessary information on a screen for the operator. A new touch sensitizer display screen has been developed to allow the computer to display push buttons on the display screen, thus allowing the operator to merely touch the screen at the appropriate point to control the experiment. Not only is this efficient for interface between the operator and the machine, but it allows the interface to be readily reconfigured as experimental needs dictate by simply reprogramming the computer.

This touch screen is now available commercially as this picture shows. (Figure 25) The John Fluke Company offers it as a keyboard for instrument


1720A INSTRUMENT CONTROLLER
CONTROLLING TEST
INSTRUMENTATION IS EASY
I'LL SHOW YOU HOW.
PRESS HERE
FLUKE

IEEE-488 bus controller
Transparent keyboard
Why BASIC
Smart temperature scanner
Portable 5½-digit computing DMM

Figure 25

control.

As the fusion program evolves, the needs of the technology program also change to respond to the new knowledge that has been gained. Examples of this response are the development of an RF technology program in support of the increased emphasis on RF applications to confinement devices. A second example of this is the increased interest in reduced activation materials for future reactors to reduce or eliminate high activation waste disposal.

As this latter example illustrates, materials are a critical element in the fusion program. In order to pursue the development of new materials for future fusion devices, facilities for the production of intense neutron sources are required. The Fusion Materials Irradiation Test Facility (FMIT) is one such facility.

The FMIT will provide the capability to determine the effects of the fusion reactor neutron environment on candidate reactor materials and will provide the basis and confirmation for materials selection for mid-term and long-term facilities of the fusion program. The objective of the FMIT project is to design, construct, equip and put into operation a facility for conducting neutron irradiation testing in an environment comparable to that in a fusion reactor. The major objectives are to determine the effects of high energy neutrons on mechanical properties of materials, the bulk irradiation effects, and surface effects and to utilize this experience to link high fluence damage from fission reactors with calculated damage at the same fluence, but with a fusion spectrum.

The FMIT project is presently completing R&D and final design work. Major construction work has been delayed for the last 2-1/2 years. OFE is actively seeking international participation in order to complete the construction of FMIT and cooperate in the subsequent experimental program. An international assessment of the FMIT project has just been completed. The results of this assessment were very favorable. A decision by the international community is expected later this year.

Applied Plasma Physics

Let me turn now to the programs supported by the Division of Applied Plasma Physics. In these programs theoretical ideas, experimental techniques and data, new diagnostic techniques, and advanced confinement concepts are developed to the point where they can contribute directly to advancing fusion development. The division also manages the magnetic fusion energy computer network, which provides the large- and medium-scale scientific and engineering computing required by the fusion program.

The fusion theory program made progress in several of the eight physics issues introduced above. In the area of fueling, an emerging idea is to use fuel ions with polarized nuclear spin to enhance the fusion reaction rate and to control the direction of emission of the fusion reaction products. The point here is that the reaction rate of colliding deuterons and tritons is very dependent on whether the quantum mechanical spins of the particles are parallel or antiparallel. In a magnetic field one can line up these spins and "freeze" them so that the whole plasma is polarized. If polarized fuel works, we might be able to improve the economics of fusion reactors, reduce the radiation hazards, and simplify the engineering. Of course, experiments will be required to verify these promising theoretical possibilities, and the experimental research for the development of spin source technology, etc., undertaken.

In the areas of equilibrium, stability, and perpendicular transport, theoretical advances occurred which will benefit tokamaks, stellarators, and mirrors. For the tokamak, research on cross-section shaping has discovered shapes which have the potential of operating in a region of much higher stable beta than conventional tokamaks. Theorists denote this the second stability region. It was also discovered theoretically that stellarators could operate in the second stability region, and with proper design could access this region more easily than tokamaks. This discovery has been incorporated into the design of the ATF-1 stellarator. The first successful tandem mirror equilibrium code was developed this year, with the aid of Japanese theorists, using ideas from the tokamak and stellarator theory. Improved understanding of mirror stability and transport led to the redesign of MFTF-B.

The Advanced Fusion Concepts program is devoting most of its resources to developing compact fusion concepts. Their small size and high power density could offer economic and engineering benefits. Included among the compact concepts are the Reversed-Field Pinch (RFP) and the Compact Toroid (CT). While related in many ways to tokamaks, RFP's are high current toroids with a magnetic field profile which allows high beta and high power density. CT's likewise allow high beta and high power density; they are further simplified by the absence of magnetic coils linking the toroidal plasma.

Progress on several physics issues of compact systems has been good over the last year. The most important results have been successes in advancing the understanding of equilibrium and stability. These successes have led to considerable increases in the stable plasma lifetime: a factor of ten in ZT-40M (an RFP); a factor of five in CTX (a CT with a toroidal magnetic field); and a factor of four in FRX-C (a CT with no toroidal field). The improvement in RFP lifetime was caused by the recently identified "dynamo" effect which maintains the proper stabilizing combination of toroidal and poloidal magnetic fields for much longer than normal diffusion lifetimes by regenerating the toroidal magnetic field for times longer than previously thought possible. Improvements in CT lifetimes were achieved in part by the dynamo effect, and in part by stabilizing magnetic windings to control instabilities which had previously limited the lifetime. Theoretical work has identified techniques which may allow true steady state operation of both RFP's and CT's.

Formation and self-heating is another area of significant progress during the last year. A reactor-relevant formation technique requires low voltages and slow formation. In ZT-40M the toroidal current risetime was successfully extended without deleterious effects by a factor of fifteen compared with the times in the earlier ceramic vacuum vessel and by six with respect to those in the metal vessel. Likewise, the CTX which employs a magnetized coaxial gun source, has been successfully operated in a long pulse mode, increasing the formation time by a factor of fifty. Moreover, the S-1, a major new facility that recently went into operation, has demonstrated an electrodeless CT formation technique with the potential for very slow startup.

One of the attractive potential reactor features of the RFP is ohmic heating to ignition. Ohmic heating alone has achieved well over 300 eV in ZT-40M this year with only a third of the rated toroidal current, and has shown that temperature scales roughly as the current. In the FRX-C, plasma heating largely by magnetic flux dissipation and adiabatic compression has produced ion and electron temperature exceeding 600 and 200 eV, respectively.

Alternate fuel cycles, which eliminate the need for tritium breeding, are of considerable interest to the fusion program. This past year we have looked at using alternate fuels in several confinement concepts. The good news is that a deuterium cycle may be possible in a tandem mirror and a reversed-field pinch. In a mirror the lower reaction rate is made up for by direct conversion of the charged particle energy into electrical energy. The bad news is that the very clean proton cycles do not appear feasible with today's understanding. Accordingly, we have terminated development of the multipole concept (Surmac at UCLA and the University of Wisconsin Octupole and supporting studies), because its major justification was based on utilization of proton fuel cycles.

The Experimental Plasma Research program brought the TEXT tokamak into full operation this year. Already several new diagnostics have been installed on this machine for final development before being deployed in the confinement experiments. These include the first local measurement of the magnetic field, which is crucial to the issues of transport and stability, and a multi-point measurement of plasma fluctuations, which will help us to understand how turbulence affects perpendicular confinement.

For many years diagnostics for direct measurement of the tokamak current density were an unfulfilled requirement. Now there are three diagnostics in final development to do this, two using atomic beams, the third using a far-infrared laser. Far infrared lasers were also used to measure ion temperature this year. Previously, the ion temperature was determined from charge exchange, which has limited resolution and is unsatisfactory in large experiments.

Addressing the stability issue, the second stability region mentioned above is being studied experimentally in a short pulse tokamak, the Torus II, at Columbia University. Stable equilibria with average beta exceeding 15% have been created and maintained. Extension of the pulse length by a factor of three has recently been accomplished. This will help us to determine the relevance of these results to conventional tokamaks.

The MFE Computer Network continues to meet the large and medium scale computing needs of the fusion program. During the past year communications costs were cut by 50% and reliability increased substantially by shifting to satellite links for the wide bandwidth circuits. The increased need for engineering, computing and data analysis was met by installing a second Cray-1 computer and by upgrading smaller computers at the major laboratories.

To provide faster, cheaper and more reliable experimental design we are installing Computer Aided Design Systems at the major sites this year, linked to the network and backed by the large computers for engineering analysis. A major step in this has been the modification of the NASTRAN stress analysis code to run on the Cray computers.

The MFE Computer Network has become indispensable to all parts of the magnetic fusion program. It is widely praised as a model for multi-institutional, programmatic computing.

FY 1984 Budget

With this strategy and general programmatic status discussion, I will now turn to the Fiscal Year 1984 budget request. The budget request for the magnetic fusion energy program in FY 1984 is $467 million as shown in Figure 26. This represents an increase of 4.6 percent over the FY 1983 level of $447.1 million. At this funding level, support is provided for developing the two mainline concepts, tokamaks and tandem mirrors, as well as the associated engineering development. In addition, support is provided for developing alternate concepts on a smaller scale. The proposed program will allow us to address key physics issues that need to be resolved in order to prepare for the concept selection, and to proceed confidently with decisions on the construction of an engineering test reactor or similar device(s). The critical reactor design and technology issues for such a reactor will also be explored. These technology issues include tasks in the areas of plasma production technologies, nuclear technologies, and system technologies. Existing facilities, as well as facilities currently under construction, will be used to pursue key technical issues of confinement physics and engineering development.

The magnetic fusion budget is organized into five interrelated functional areas that form the basis of a unified and balanced effort in conducting the research and development necessary to define a practical fusion product. The Confinement Systems (CS) and Applied Plasma Physics (APP) sub-programs working in conjunction, provide the physics basis for the fusion program. In the near term, the primary driving force of the program is derived from the experiments conducted in the CS sub-program. These results are reviewed and analyzed by the APP sub-program, with the result being development of plasma physics

ENERGY RESEARCH

MAGNETIC FUSION ENERGY
(B/A in Millions)

	FY 1982	FY 1983*	FY 1984
Operating	$295.1	$361.6	$392.7
Capital Equipment	42.0	39.5	37.8
Construction	114.1	46.0	36.5
Total MFE	$451.2	$447.1	$467.0

* Continuing Resolution

Figure 26

theories and models to explain the experimental results and suggest directions for additional investigations. Based on this information, the APP subactivity also initiates experimental work and theoretical investigations of alternate confinement concepts on basic physics problems and develops diagnostic instruments for use by the CS sub-program. Based on the program's physics results and projections, the Development and Technology (D&T) sub-program develops components and systems for use in near and mid-term fusion experiments and performs design studies of fusion power reactor types which most likely will eventually be built. These design studies serve to identify component and system development needs that must be undertaken to prepare for the eventual commercialization of fusion. In carrying out the assignments of the physics and technology tasks, needs for major new facilities are identified from time to time. The Planning and Projects (P&P) sub-program is responsible for constructing these major new facilities. Finally, the Program Direction sub-program provides the funding to cover salaries, benefits, and travel expenses associated with the requirement of staffing the Headquarters Office of Fusion Energy staff. The proposed budget levels for each of these areas are indicated in Figure 27.

The budget request for fusion reflects the Administration's continued commitment to develop the highest potential of fusion energy in an environmentally acceptable and economically competitive manner. The budget also recognizes the fact that, given overall budget stringency, funding for fusion development will be essentially flat. Even so, progress towards achieving scientific and engineering feasibility by the turn of the century can be maintained. Some hard choices have been made in the proposed programs

Magnetic Fusion Energy
FY 1984 Congressional Budget
Program Element Total
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request
Magnetic Fusion Energy			
Applied Plasma Physics			
Operating expenses	$ 66.8	$ 73.3	$ 76.0
Capital equipment	6.2	4.0	5.1
Subtotal	73.0	77.3	81.1
Confinement Systems			
Operating expenses	123.7	181.0	186.0
Capital equipment	18.2	21.8	20.7
Construction	13.5	0.0	0.0
Subtotal	155.4	202.8	206.7
Development and Technology			
Operating expenses	70.0	74.3	77.3
Capital equipment	12.7	9.7	7.8
Construction	6.0	0.0	0.0
Subtotal	88.7	84.0	85.1
Planning and Projects			
Operating expenses	31.0	29.0	48.7
Capital equipment	4.9	4.0	4.3
Construction	94.6	46.0	36.5
Subtotal	130.5	79.0	89.5
Program Direction			
Operating expenses	3.6	4.0	4.6
Subtotal	3.6	4.0	4.6
Total Magnetic Fusion Energy	$451.2	$447.1	$467.0

Figure 27

recognizing fully the inherent benefits and risks associated with a level budget. These include such actions as shutting down and modifying facilities, recognizing that FMIT can proceed only as an international project, focusing technology programs on critical feasibility issues, and increasing our utilization of international programs.

At the proposed level the continued emphasis will be along scientific and base technology lines. As indicated in Figure 28, the operating budget request represents an increase of $31.1 million. This increase is primarily associated with the funding required for fabrication of auxiliary components on the MFTF-B. Funding of this fabrication work is necessary so that the components can be available for installation in a timely fashion for initial operation. MFTF-B accounts for about half ($16.7 million) of the total operating increase. However, this increase is partially offset by a $10 million decrease in the MFTF-B construction account. The balance of the operating program is provided with a net increase over FY 1983 of about 4 percent. The increase in operating is partially offset by a decrease of $1.7 million in capital equipment and $9.5 million in construction, as several projects are nearing completion and no new projects are proposed. Also reflected in the proposed budget is a decision to terminate the Elmo Bumpy Torus Proof-of-principle (EBT-P) project. Within the proposed funding level of $467 million, and with the expectation of flat budgets in the near term, it was determined that the attendant programmatic consideat this time with the completion of this project. Funding priorities and technical developments will determine whether a next step EBT-P-type device can be reconsidered in future years. A base scientific program to extend the physics understanding

Magnetic Fusion Energy
FY 1984 Congressional Budget
Program Totals
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request	Change
Operating Expenses	$ 291.5	$ 357.6	$ 388.1	$ 30.5
Capital Equipment	42.0	39.5	37.8	(- 1.7)
Construction	114.1	46.0	36.5	(- 9.5)
Program Direction	3.6	4.0	4.6	0.6
Total	$ 451.2	$ 447.1	$ 467.0	$ 19.9

Figure 28

of this alternative concept will be maintained using the EBT-S facility.

With this general introduction, let me now turn to a more detailed description of the proposed program.

Applied Plasma Physics

The APP sub-program provides for research in the area of Advanced Fusion Concepts, Experimental Plasma Research, Fusion Theory, and the Magnetic Fusion Energy Computer Network (MFECC). Each of these areas has its own role to play in the CS sub-program. The first is to develop advanced fusion concepts that have the potential for improved reactor configurations and that contribute to the fusion "idea-pool". Secondly, experimental plasma research initiates new capabilities, develops new diagnostics, and provides physics design data for mainline concepts. Third, fusion theory provides the theoretical underpinnings and idea generation for the entire fusion program. The fourth provides for the operation and maintenance of the MFECC. As indicted in Figure 29, the total request for these activities in FY 1984 is $81.1 million, including $76 million for operating and $5.1 million for capital equipment.

In Advanced Fusion Concepts, an increase of $1.3 million is requested over the FY 1983 level. The FY 1984 funding level will allow for operation of the S-1 spheromak at PPPL, maintenance of operation on the reversed field pinch (ZT-40) device at LANL, and operation of the compact toroid experiments. Funding is provided to initiate a major device fabrication project (ZT-H) at LANL to extend the performance of the reversed field pinch machine. Studies on the smaller scale modular stellarator experiment at the University of Wisconsin will continue. The pace of development on these alternative

Magnetic Fusion Energy
FY 1984 Congressional Budget
Applied Plasma Physics
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request	Change
Operating Expenses				
Advanced Fusion Concepts	$ 17.4	$ 18.6	$ 19.9	$ 1.3
Experimental Research	15.2	16.9	17.1	0.2
Theory	21.0	22.3	23.3	1.0
Computer Network	13.2	15.5	15.7	0.2
Subtotal	66.8	73.3	76.0	2.7
Capital Equipment	6.2	4.0	5.1	1.1
Total Applied Plasma Physics	$ 73.0	$ 77.3	$ 81.1	$ 3.8

Figure 29

concepts will be determined by technical results, available resources, and perceived programmatic benefits.

In the Experimental Plasma Research category, an increase of $0. 2 million is requested. This, together with decreases resulting from the termination of some tasks, will be used to increase effort on those projects that provide information on critical issues or that are developing new measurement techniques. The TEXT user facility will be maintained at the FY 1983 level of effort to allow contractors to conduct research in a hot plasma of moderate size. Radio frequency current drive improvements will be explored on Versator II at M.I.T. and TORUS II at Columbia University and will be upgraded to allow the study of equilibrium and instabilities at high beta.

In the Fusion Theory category, an increase of $1.0 million is requested for developing and applying models of plasma behavior in all fusion devices. Critical phenomena, such as pressure limits, confinement, and impurity control are still only partially understood and necessitate intense theoretical efforts. The requested funding will support efforts on better understanding current drive techniques for tokamaks, developing coupled three dimensional equilibrium - transport models for tandem mirrors, improved understanding of dynamo processes in reversed field pinches and compact toroids, and improved understanding of ion transport and beta limits in EBT.

The MFE Computer network provides the scientific and engineering computing needs for the entire fusion program, using the appropriate mix of computer size and communication links. The large-scale computing needs are centralized at the MFECC in LLNL. Smaller scale time sharing and data analysis is done at one of five user service centers at the National laboratories. The budget request for the activity provides an increase of $0.2 million. At this level we will be able to support operation of the Center, complete installation of the computer-aided design system, develop initial links between computer aided design files and engineering analysis codes, and improve the communications network to meet the need for more nodes and higher band width.

The Capital Equipment request of $5.1 million represents an increase of $1.0 million of which about half is associated with initiating the procurement of a new motor generator set for the ZT-40 device.

Confinement Systems

The activities under this sub-program are aimed at demonstrating and refining methods of heating and containing high temperature plasmas in toroidal and mirror systems. These include the two mainline concepts, tokamaks and tandem mirrors. While tokamaks are the most developed concept, both represent near-term fusion reactor candidates. Tokamaks and mirrors both have medium size devices in operation or under construction that have demonstrated or will soon test the basic principoles of the two concepts. Also included is funding for scientific development of the EBT alternate concept.

As indicated in Figure 30, a total of $159.7 million is requested in the toroidal area, including $63.6 million for research operation support, $8.2 million for major device fabrication, $72.3 million for TFTR, and $15.6 million for capital equipment. Overall, this represents an increase of $6.3 million.

Magnetic Fusion Energy
FY 1984 Congressional Budget
Confinement Systems - Tokamaks
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request	Change
Operating Expenses				
Research Operations	$ 49.3	$ 55.6	$ 63.6	$ 8.0
TFTR	35.7	72.1	72.3	0.2
MDF's	3.7	10.0	8.2	(- 1.8)
Subtotal	88.7	137.7	144.1	6.4
Capital Equipment				
TFTR	5.7	7.7	9.7	2.0
Other	7.3	7.9	5.9	(- 2.0)
Subtotal	13.0	15.6	15.6	0
Total Confinement Systems - Tokamaks	$ 101.7	$ 153.3	$ 159.7	$ 6.4

Figure 30

The Research Operations activity primarily provides support for the operation of four experimental tokamak devices to demonstrate that confinement, beta values, and reactor-relevant current drive are scalable to an ETR. The funding request includes $17.2 million for PLT, $8.0 million for Alcator C, $9.0 million for ISX-B, and $25.5 million for Doublet III (D-III), including physics support, the new "Big D" vacuum vessel. Experiments on current drive which lead to a steady state reactor will be carried out at high power levels on Alcator C at M.I.T. More exploratory studies to develop new current drive techniques will be conducted on PLT at PPPL, where ICRF heating development will also continue. With the shutdown of PDX in FY 1983, impurity and disruption control will be carried out on ISX-B at ORNL in FY 1984. The Doublet III device, operated by GA Technologies, will conduct studies on various cross-sectional plasma shapes at near reactor conditions using the complement of three beamlines. A joint international program with the Japanese Atomic Energy Research Institute is also being conducted on this machine to help on efforts in achieving reactor level beta values in elongated discharges. An additional $3.9 million is requested for exploring advanced toroidal ideas such as the advanced Toroidal Facility at ORNL.

Support for three ongoing projects will continue. The D-III new vacuum vessel project will maximize the usefulness of the D-III facility by replacing the present doughnut-shaped vessel with a larger dee-shaped vessel. This will become the prime facility on which basic tokamak research is conducted in the period 1986-1990. The budget includes $4.5 million for this upgrade. In addition, the PLT facility will continue to be modified to provide for steady state operation to allow low power current drive experiments at reactor level densities. Funding of $2.7 million is requested for this activity. Finally, the installation of a multi-megawatt ECH system on D-III will continue to provide a facility for testing the usefulness of EC for bulk plasma heating and profile modificatin. This effort is funded at a level of $2.0 million. The TFTR device will begin in FY 1984 to provide scaling information during its first full year of operation based on ohmic discharges at about 50 percent power levels. The TFTR neutral beam heating phase will begin preparation for full power beam operation and machine optimization in 1985 and for the demonstration of scientific feasibility of fusion in a tokamak in 1986. The funding request for TFTR in FY 1984 is allocated to five categories, including: $11.9 million for diagnostics; $9.0 million for the core physics staff; $7.4 million for the CICADA computer control systrem; $22.3 million for facility operation; and $21.7 million for technical support related to testing and bringing neutral beams on-line.

A total of $15.6 million is requested for capital equipment support in toroidal confinement systems. This is equal to the amount provided in FY 1983 for this account. Of this total $9.7 million is for TFTR and $5.9 million is associated with other toroidal projects.

As indicated in Figure 31, a total of $47.0 million is requested for the Mirror Confinement System area; including $38.7 million for research operations, $3.2 million for major device fabrications, and $5.1 million for capital equipment support. The total request for this category represents an increase of $2.5 million or about 5 percent over FY 1983. The funds in this category support the development of the mainline tandem mirror concept and the Elmo Bumpy Torus alternative concept. The tandem mirror effort accounts for

Magnetic Fusion Energy
FY 1984 Congressional Budget
Confinement Systems - Mirrors
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request	Change
Operating Expenses				
Research Operations	$ 27.3	$ 33.3	$ 38.7	$ 5.4
EBT-P	4.0	4.0	0.0	(- 4.0)
MDF's	3.8	6.0	3.2	(- 2.8)
Subtotal	35.1	43.3	41.9	(- 1.4)
Capital Equipment	5.2	6.2	5.1	(- 1.1)
Construction				
EBT-P	13.4	0.0	0.0	0.0
Total Confinement Systems - Mirrors	$ 53.7	$ 49.5	$ 47.0	$(- 2.5)

Figure 31

$38.2 million of the FY 1984 total, while the EBT is funded at a level of $8.8 million.

The tandem mirror research program will focus effort on investigating the physics of the thermal barrier, sloshing ion distributions, microinstabilities, and negative potential operating modes in tandem mirrors. The research operating budget request of $30.2 million for tandem mirrors will support the operation of the TMX-Upgrade device at LLNL which will be the primary "workhorse" for the tandem mirror program. Funding of $20.0 million for this device in FY 1984 will be used to complete radial transport studies, investigate negative potential operation mode, test center cell beta limits, and optimize the startup and operating modes of thermal barrier tandem mirrors. This Tara device at M.I.T. is provided $4.6 million to initiate experimental operations to test and compare tandem mirror operation with hot electron anchors versus hot ion anchors. Several smaller mirror experiments will be used to support TMX-U and provide first tests of new ideas. Also included under Research Operations is $8.5 million to support the operation of the EBT-S facility at ORNL and for related studies. This will allow us to maintain a base scientific program on this alternate concept. During FY 1984, efforts will include installation and operation of improved plasma diagnostics to study optimized confinement, conduct scaling experiments, and investigate alternative heating configurations. As indicated previously, no funds are request for the EBT-P project.The funding request of $3.2 million for the Major Device Fabrication category provides for completion of the Tara axisymmetric tandem mirror project. Capital equipment support will be provided at the level of $5.1 million to support on-going mirror program efforts.

Development and Technology

The goals of the Development and Technology sub-program are to provide technology support to the major existing confinement experiments, to develop the critical engineering and technological basis needed for selection of the most attractive confinement and technology options for the ETR, and to support key long-term engineering requirements for development of commercial fusion.

As indicated in Figure 32, the FY 1984 budget request of $85 million will allow us to focus efforts on the critical "go-no-go" feasibility issues using existing facilities and increased reliance on international cooperation. Highest priority will be put on proceeding with tasks associated with achieving technology readiness for ETR concept selection, and resolving issues of fusion power feasibility. These tasks include work in the area of plasma heating, plasma fueling, impurity and exhaust; first wall/structure/high heat flux materials; blanket/shield systems; superconducting magnets; and remote maintenance.

The budget request for Development and Technology represents an increase of $1 million over FY 1983 and includes $77.3 million for operating and $7.7 million for capital equipment. Based on physics results and project ions, this funding level will permit the sub-program to develop components and systems for near and mid-term experiments, perform engineering design studies of practical reactor sysstems, and investigate critical technologies required by these systems to ensure their ultimate feasibility.

The D&T operating budget request includes $16.8 milion for the magnetic

Magnetic Fusion Energy
FY 1984 Congressional Budget
Development and Technology
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request	Change
Operating Expenses				
Magnets	$ 14.9	$ 16.4	$ 16.8	$ 0.4
Plasma Engineering	15.3	18.6	17.5	(- 1.1)
Reactor Materials	15.1	16.8	19.0	2.2
Fusion Systems Engineering	11.0	12.6	11.9	(- 0.7)
Environment and Safety	2.7	2.8	3.0	0.2
Fusion Energy Applications	1.8	1.6	1.0	(- 0.6)
Engineering Reactor Design and Development	9.2	5.5	8.1	2.6
Subtotal	70.0	74.3	77.3	3.0
Capital Equipment	12.7	9.7	7.7	(- 2.0)
Construction				
LCTF	6.0	0.0	0.0	0.0
Total Development and Technology	$ 88.7	$ 84.0	$ 85.0	$ 1.0

Figure 32

systems category. These funds will be used to develop and demonstrate the technology that will enable timely construction of reliable, cost effective superconducting magnets. In FY 1984, the two remaining U.S. large coils will be completed and installation will start for the six coil test in the LCTF.

In the Plasma Engineering category, $17.5 million is requested to develop and demonstrate the components and systems necessary for plasma heating, fueling and impurity control. Plasma heating issues being addressed in FY 1984 include steady-state, high energy, high power negative ion neutral beams, and radio frequency systems. Plasma fueling and exhaust issues being emphasized include pellet and gas injection and limiter systems for exhaust.

The Fusion Reactor Materials category is requesting a funding level of $19 million. The objective here is to identify and develop materials, including structural and non-structural metals, ceramics, polymers, and tritium breeding materials which are required to build and support experimental plasma test facilities and fusion power reactors. Studies on the effects of high neutron, particle and heat fluxes will be conducted. The RTNS-II will conduct material tests at full capacity with operation of two beam lines. Operation of the Oak Ridge reactor will be supported. Initial test of high heat flux components at the Plasma Materials Test Facility will be completed. A program to address the potential impact of utilizing low activation materials in fusion reactors will be initiated in FY 1984.

In the Fusion Systems Engineering category fusion reactor engineering issues will be addressed which are associated with reactor concept selection and are critical to practical fusion energy development. Reactor systems studies will guide physics and engineering developments towards practical reactor concepts. The FY 1984 funding level of $11.9 million provides for continuation of tritium handling activities at the FY 1983 level, while permitting an increase in blanket and shield engineering to address critical issues in blanket breeding and power conversion. Initial tritium processing and cleanup tests in TSTA will be completed. The reactor studies effort will focus on conceptual design of fusion power reactors as input to the reactor concept selection process and on engineering studies of critical issues. Design studies to select the best blanket concepts for further development will be completed.

A budget of $3.0 million is requested for the Environment and Safety category to carry out tasks required to ensure that the development and ultimate operation of fusion power reactors are accomplished with minimum adverse effects in the environment. The final version of the Generic Environmental Impact Statement for Magnetic Fusion programs will be issued. The Fusion Energy Applications category will be funded at $1 million. This will primarily be used on the synfuels program for studying improvement to present thermochemical cycles and explore alternate cycles such as ethanol productions.

The Engineering Reactor Design and Development category is requesting $8.1 million. This category has two primary objectives in FY 1984. The first is to estimate cost, performance, and uncertainties of various strategies for achieving engineering feasibility. Emphasis will be on reference designs of advanced mirrors and tokamak ETR systems. The second objective is to compare the attractiveness of tokamak and mirror reactor confinement concepts. In FY 1984, the ETR pre-conceptual designs will be completed.

The request for capital equipment to provide support to the D&T sub-programs is $7.7 million, a decrease of $2 million from the $9.7 million in FY 1983. A portion of this decrease is due to the completion of LCTF.

Planning and Projects

The Planning and Projects sub-program is responsible for constructing major new facilities and conducting project-specific research and development activities. General Plant Projects and General Purpose Equipment are also funded in this category. As indicated in Figure 33, the FY 1984 budget request for this sub-program is $89.6 million, which includes $48.7 million in operating, $4.4 million in capital equipment, and $36.5 million in construction.

The major effort in FY 1984 is associated with construction of the MFTF-B and fabrication of auxiliary components for the facility. MFTF-B is a large thermal barrier tandem mirror device designed to operate at near breakeven conditions. The budget request includes $30 million for construction and $25 million for operating. This will allow us to complete the winding of all magnets and begin installation of the magnets. Fabrication of the vacuum vessel will be completed and installation of the vacuum system and cryogenic system will begin. MFTF-B is scheduled to be operational in FY 1984 and will provide a bridge in the physics and technology gap between TMX-U and a large D-T burning test reactor.

Funding is also provided to initiate a joint international cooperative effort on the FMIT. The budget request for this project is $3 million in operating funds. This facility, when complete, will allow us to conduct neutron

Magnetic Fusion Energy
FY 1984 Congressional Budget
Planning and Projects
B/A in Millions

	FY 1982	FY 1983	FY 1984 Request	Change
Operating Expenses				
TFTR/TFM	$ 17.0	$ 17.5	$ 20.7	$ 3
MFTF-B	9.0	8.3	25.0	16
FMIT	5.0	3.2	3.0	(- 0.2)
Subtotal	31.0	29.0	48.7	19.7
Capital Equipment				
TFTR	1.6	0.0	0.4	0.4
GPE	3.3	4.0	4.0	0.0
Subtotal	4.9	4.0	4.4	0.4
Construction				
TFTR	40.1	0.0	0.0	0.0
MFTF-B	39.9	40.0	30.0	(-10.0)
FMIT	9.0	0.0	0.0	0.0
GPP	5.7	6.0	6.5	0.5
Subtotal	94.7	46.0	36.5	(- 9.5)
Total Planning and Projects	$ 130.6	$ 79.0	$ 89.6	$ 10.6

Figure 33

irradiation testing in the environment comparable to that of a fusion reactor. The requested funding will provide for development efforts on the accelerator, and remote handling and experimental servicing equipment.

Funding is also provided for continuation of the TFTR flexibility modification project to allow self-heating of the plasma to occur as a result of burning fusion fuel, deuterium and tritium. This effort is funded at a level of $20.7 million. These funds will allow fabrication of the vacuum vessel armor plating and surface pumping system to continue. Also provided for is design, fabrication, and installation of radiation shielding and fire seals for TRM power levels. The capital equipment request of $4.4 million provides $0.4 million to support TFM activities and $4 million to support Energy Research's landlord responsibilities at ORNL.

In addition, $6.5 million is requested for General Purpose Plant projects. Based on current plans, $4.2 million will be provided to ORNL, $1 million to LLNL, $1 million to PPPL, and $0.3 million to LANL.

Program Direction

The FY 1984 budget request for magnetic fusion energy Program Direction is $4.6 million. These funds are required to provide for the salaries, benefits, travel, and other expenses associated with the 71 full-time equivalents required to administer the program.

Figure 34 lists the six principal fusion R&D centers, along with their emphases and budget. With more detail, Figure 35 shows all the major participants—defined as those organizations receiving more than one million dollars of support annually.

Principal Fusion R&D Centers

Major Contractors	Emphasis	Total FY 84 Budget (BA $ in millions)	Percent
Princeton	Tokamaks	134.7	29
Lawrence Livermore	Mirrors	109.5	23
Oak Ridge	Tokamaks, EBT, Technology	67.0	14
MIT	Tokamak, Mirror	23.2	5
GA Technologies, Inc.	Tokamak	38.5	8
Los Alamos	Alternate Concepts	21.5	5
Others*		50.0	11
Universities		22.6	5
		467.0	100%

*Includes: ANL, BNL, LBL, HEDL, PNL, INEL, SANDIA, Direct Industrial Contracts (excluding GA) and Program Direction

Figure 34

MAJOR FUSION PROGRAM PARTICIPANTS

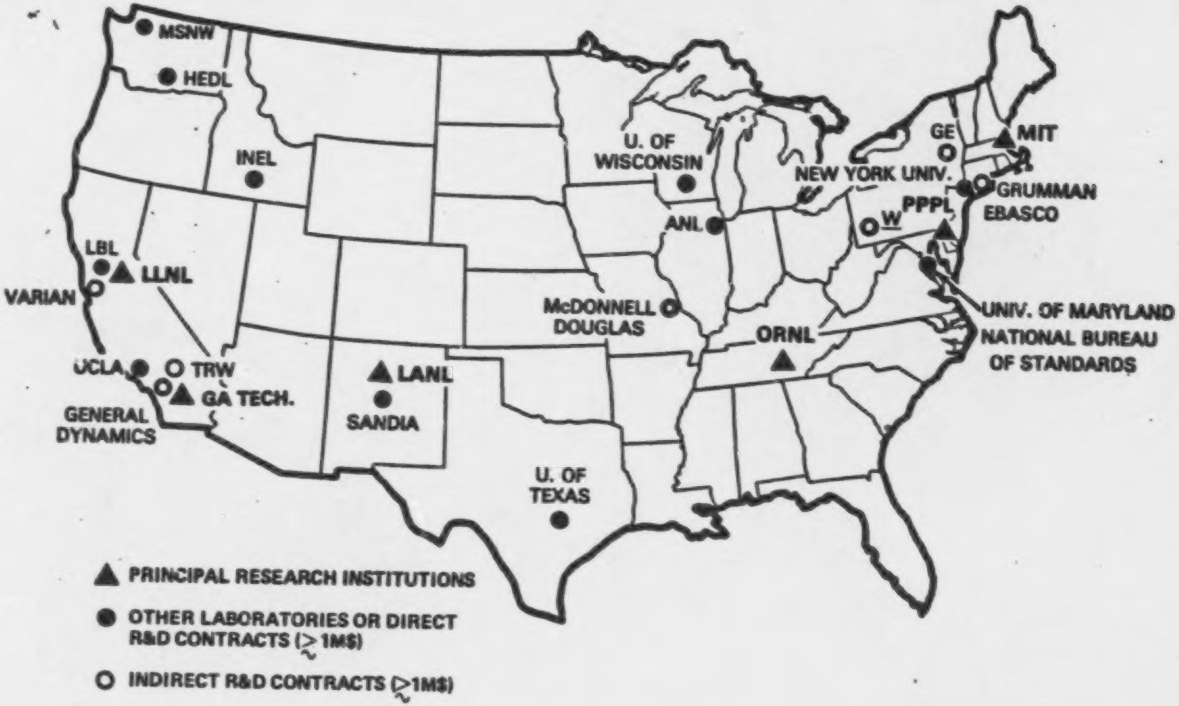

MSNW
HEDL
INEL
LBL
LLNL
VARIAN
UCLA
TRW
GA TECH.
GENERAL DYNAMICS
LANL
SANDIA
U. OF TEXAS
U. OF WISCONSIN
ANL
McDONNELL DOUGLAS
ORNL
GE
MIT
NEW YORK UNIV.
GRUMMAN
EBASCO
W
PPPL
UNIV. OF MARYLAND
NATIONAL BUREAU OF STANDARDS
▲ PRINCIPAL RESEARCH INSTITUTIONS
● OTHER LABORATORIES OR DIRECT R&D CONTRACTS (≳ 1M$)
○ INDIRECT R&D CONTRACTS (≳1M$)

Figure 35

As a last point, I want to show you the organizational arrangement (Figure 36) I use to manage the program in the Department, including the individuals who form the management team. The fusion program has been and continues to be characterized by a strong, central management, composed of technically strong Headquarters personnel working closely with the laboratory leaders and supported by project management groups, and more recently, program management support as well in the Department's field offices. I am pleased to conclude this testimony by noting that I represent a program that is based on a broad involvement by the fusion community in all aspects of program planning and implementation.

Thank you.

Mrs. BOUQUARD. Thank you. Since we do have so many new members I appreciate your introductory slides. I think it might be well if you would be equally generous to the new members, as was the courtesy shown to me a few years ago, and meet with our new members if you have not done so.

Dr. TRIVELPIECE. I would be pleased to do so.

Mrs. BOUQUARD. We would appreciate it if you would review the basics of the program. We certainly do want to have a very hard-working subcommittee this year and they are a very active and a very bright group and I think, as a result, we're going to have a good subcommittee. I'm certainly looking forward to working with Mr. Walker and the others this year.

What's the administration's position on the additional funds that were appropriated through the magnetic fusion energy program for 1983? It's my understanding that our appropriation committee's level is about $20 million over the administration's request. Is this money to be released or is the administration considering rescission or reprograming of these funds?

Dr. TRIVELPIECE. I don't mean this to be a complaint but, of course, we have only recently received the letter from Mr. Bevill informing us of the committee's intent with respect to the use of these funds. It is, I believe, $22 million above the President's fiscal year 1983 congressional budget request; $10 million was allocated for operating expenses for MFTF-B; $2 million was for construction for FMIT; $10 million was for construction for the EBT-P project.

Mrs. BOUQUARD. Would you mind going over those again?

Dr. TRIVELPIECE. $10 million for MFTF-B operating; $2 million FMIT construction; and $10 million EBT-P construction. Now, with respect to the $10 million of MFTF-B operating funds, our plans, at the moment are to use $7 million for the MFTF-B project. The other $3 million will be directed into FMIT operating. We will use $2 million of construction money also on that project.

With respect to EBT-P, you know, there has been a technical review and we did not request 1984 funds for construction. We believe there should be an EBT-P program at Oak Ridge, but we do not believe that we should proceed with the construction at this time. We will have to make a proposal back to you that is consistent with that and we haven't done so yet.

Mrs. **BOUQUARD**. OK. You had $10 million for EBT-P in the 1983 money?

Dr. **TRIVELPIECE**. Yes, we have $10 million in construction funds. The Department has yet to make a decision with respect to the utilization of these funds.

Mrs. **BOUQUARD**. This is the $10 million that Mr. Bevill spelled out in his letter that he wished you would proceed with for fiscal year 1983. But you have not made a decision yet?

Dr. **TRIVELPIECE**. We have made a tentative decision not to proceed with the construction consistent with our 1984 request, but to continue with an EBT research program.

Mrs. **BOUQUARD**. What activities will DOE carry out in 1983 with this funding?

Dr. **TRIVELPIECE**. I believe it is necessary for us to make a concrete proposal to you regarding the utilization of these funds and we have not had time to do that yet.

Mrs. **BOUQUARD**. When do you think you will reach a decision?

Dr. **TRIVELPIECE**. I would certainly hope within the next month. I see no reason it should take any longer than that.

Mrs. **BOUQUARD**. So you can't perceive exactly what your activities will be then for the 1984 budget since you haven't decided what you're going to do for 1983.

Dr. **TRIVELPIECE**. As I indicated in my earlier comments, we only received the information recently that the Congress intended that there should be $10 million in construction activity and we haven't had time yet to analyze the various possibilities. But yes, that may have some impact on the 1984 budget as well.

Mrs. **BOUQUARD**. I was going to inquire about the DOE's position on the suggested TFCD facility. Where do you think the conceptual designs should be done? Do you think that we are going to have competing design teams? What funding is requested in 1984 for this activity; that is, the fusion core demonstration [TFCD] facility?

Dr. **TRIVELPIECE**. This is what I was referring to before when I indicated that Dr. Davidson was going to mention in his remarks with respect to the Magnetic Fusion Advisory Committee. I don't believe things have progressed to the stage yet at which that question has been addressed or a conclusion reached as to what kinds of studies will be carried out and where. But that is my understanding.

Mrs. **BOUQUARD**. One final question. You might know that we did have significant criticism yesterday from elements of the university community in reference to the national labs interactions with the universities. I was wondering if the applied plasma physics program is the funding area whereby the DOE chiefly supports university efforts, is that correct?

Dr. **TRIVELPIECE**. Yes, I believe that is correct.

Mrs. **BOUQUARD**. Couldn't we solve it by encouraging more joint laboratory-university research or additional funding directly to the universities.

Dr. **TRIVELPIECE**. I am a little at a loss to understand quite what the problem is, although I will look into it. The fusion program has direct support for university programs of about $45 million. That amount is about a sixth of the total amount that the Department of Energy puts in universities, which is about $280 million. That's the second largest single mission oriented support activity for universities in the United States and I suspect in the world. So I'm not quite sure why the university programs are concerned.

Mrs. Bouquard. This is an area we need to look into too. If I remember correctly, this is about twice the figure we heard yesterday.

Dr. Trivelpiece. If you like, I would be glad to supply the figures for the record. However, I'm reasonably confident about that amount.

Mrs. Bouquard. $27 million is the figure we were quoted yesterday.

Dr. Trivelpiece. I don't remember the numbers for each university but I could supply it for the record.

[The information follows:]

MAGNETIC FUSION
UNIVERSITY FUNDING
Budget Authority in Thousands

	FY 1984
Arizona University of	80
Auburn University	95
California Institute of Technology	315
California University of Berkeley	640
California University of Irvine	420
California University of Los Angeles	3,913
California University of Santa Barbara	240
California University of San Diego	200
Columbia University	860
Connecticut University of	74
Cornell University	500
Dartmouth College	0
Georgia Institute of Technology	270
Illinois University of	100
Iowa University of - Iowa City	0
Johns Hopkins University	495
La Jolla Institute	60
Maryland University of	1,643
Massachusetts Institute of Technology	23,175
Michigan University of	115
Missouri University of	88
New York State University of	90
New York University	1,152
North Texas State University	0
Northwestern University	0
Pennsylvania State University	314
Rennselaer Polytechnic Institute	320
Rochester University of	0
Smithsonian Institution	0
Stanford Leland Junior University	32
Texas University of	6,355
Virginia University of	0
Washington University of	675
Wesleyan University	57
Western Ontario University of	0
William and Mary College of	86
Wisconsin University of	3,282
Yale University	115
TOTAL	$45,761

Mrs. **Bouquard**. Dr. Trivelpiece, we thought you had all these numbers in your head.

Dr. **Trivelpiece**. I could make a guess at it but I'm afraid I would embarrass myself.

Mrs. **Bouquard**. You've done a fine job. Thanks a lot.

Mr. Chandler?

Mr. **Chandler**. I have no questions.

Mrs. **Bouquard**. Mr. Walker?

Mr. **Walker**. Thank you, Madam Chairman.

I have an opening statement that I would appreciate being made a part of the record.

Mrs. **Bouquard**. Without objection your statement and Congressman Winn's statements will be made a part of the record. We're also very happy to have Mr. Winn with us today and appreciate his invaluable contribution to this subcommittee as well.

[The opening statements of Mr. Walker and Mr. Winn follow:]

OPENING STATEMENT
THE HONORABLE ROBERT WALKER

ERP SUBCOMMITTEE HEARING
MAGNETIC FUSION ENERGY

MARCH 17, 1983

THANK YOU, MADAM CHAIRMAN, FOR THIS OPPORTUNITY TO WELCOME OUR WITNESSES. MAGNETIC FUSION ENERGY PROGRAM IS A SIGNIFICANT COMMITMENT BY THE FEDERAL GOVERNMENT. WE ARE CERTAINLY INTERESTED IN HOW THE DEPARTMENT OF ENERGY IS MANAGING THIS EFFORT. DR. TRIVELPIECE, WHO IS THE DIRECTOR OF THE OFFICE OF ENERGY RESEARCH IN THE DEPARTMENT OF ENERGY, IS HERE TODAY TO DISCUSS THE PROGRAM WITH US. HIS TESTIMONY, PLUS THAT FROM WITNESSES REPRESENTING THE MAGNETIC FUSION ADVISORY COMMITTEE AND THE NATIONAL RESEARCH COUNSEL SHOULD BE VERY HELPFUL.

THE MAGNETIC FUSION ENERGY PROGRAM IS ONE OF THE NATION'S MAJOR RESEARCH EFFORTS TO DEVELOP A NEW SOURCE OF ENERGY. I BELIEVE THAT PROBABLY WE HAVE ALL HEARD FROM TIME TO TIME OF THE PROGRAM WHEN DISCUSSIONS LEAD TO THE ENERGY REQUIREMENTS OF THE FUTURE AND THE PROCESSES BEING DEVELOPED TO MEET THOSE FUTURE NEEDS. WE DO NOT EXPECT IMMEDIATE RESULTS FROM THESE SIGNIFICANT SUMS OF MONEY WE ARE COMMITTING, BUT WE DO LOOK FORWARD TO ACCOMPLISHMENTS ALONG THE LINE. WE EXPECT TO HEAR FROM OUR WITNESSES TODAY THAT PROGRESS IS BEING MADE TO THIS ENERGY GOAL, HOWEVER DISTANT IT MAY BE. THE INTELLIGENCE AND IMAGINATION WHICH THIS PROGRAM REQUIRES WILL, IN TURN, NECESSITATE THE ACCOMPLISHMENTS OF ALL OUR NATIONAL RESOURCES. SO, WE LOOK TO UTILIZING OUR NATIONAL LABORATORIES, OUR COLLEGES AND UNIVERSITIES, AND OUR INDUSTRIAL ESTABLISHMENT. WE WILL WANT ASSURANCE THAT ALL OF THESE RESOURCES ARE BEING ADEQUATELY USED AND THAT THEY ARE ABLE TO WORK TOGETHER IN EFFICIENT AND COST-CONSCIOUS MANNER.

THANK YOU, MADAM CHAIRMAN. I AM CERTAIN THAT THIS HEARING WILL BE BENEFICIAL TO US ALL.

OPENING STATEMENT
THE HONORABLE LARRY WINN, JR.

ERP SUBCOMMITTEE HEARING
MAGNETIC FUSION ENERGY

MARCH 17, 1983

THANK YOU, MADAM CHAIRMAN. I JOIN YOU IN YOUR WELCOME TO OUR WITNESSES TODAY. IT IS A PLEASURE AGAIN TO SEE DR. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY. HE WILL DISCUSS THE DOE PROGRAM ON MAGNETIC FUSION. I ALSO LOOK FORWARD TO HEARING FROM WITNESSES ON THE MAGNETIC FUSION ADVISORY COMMITTEE REPORT AND THE NATIONAL RESEARCH COUNSEL'S COMMITTEE ON MAGNETIC FUSION REPORT.

THE MAGNETIC FUSION PROGRAM EXEMPLIFIES THE KIND OF INVESTIGATION WHICH MERITS FEDERAL GOVERNMENT FUNDING. WE BELIEVE THAT SUCCESSFUL TERMINATION OF THIS TECHNOLOGY WILL REQUIRE A LONG PERIOD OF TIME. WE ARE ADVISED THAT, TECHNICALLY, THE PROGRAM HAS COME A LONG WAY BUT THERE ARE STILL A LARGE NUMBER OF TECHNICAL RISKS CONFRONTING US. ON THE OTHER HAND, SUCCESSFUL COMMERCIALIZATION OF THIS PROCESS WILL PROVIDE A TREMENDOUS PAY-OUT FOR FUNDS EXPENDED NOW. SO THE PROGRAM DOES MEET THE CRITERIA FOR FEDERAL INVOLVEMENT.

THE EXTENT TO WHICH THE PROGRAM STILL MUST GO FOR COMMERCIALIZATION WILL REQUIRE ALL THE BEST MINDS AND

FACILITIES THIS NATION CAN BRING TO BEAR ON THE PROBLEMS. THESE RESOURCES WILL NECESSARILY COME FROM THE NATIONAL LABORATORIES, OUR COLLEGES AND UNIVERSITIES, AND THE INDUSTRIAL SECTOR. WE CAN EXPECT SUCH A TRILATERAL ARRANGEMENT TO CONTRIBUTE TO SUCCESS, AS IT HAS TO OTHER NATIONAL PROGRAMS. HOWEVER, IN TESTIMONY RECEIVED BY THIS SUBCOMMITTEE YESTERDAY IN THIS ROOM, WE WERE ADVISED THAT PARTICIPATION BY INDUSTRY AND BY OUR COLLEGES AND UNIVERSITIES COULD BE VASTLY EXPANDED. IN FACT, WE WERE TOLD THAT THE USE OF THESE TWO EXTREMELY IMPORTANT SECTORS WAS MINIMAL AND IN SOME CASES APPEARED TO BE RETREATING EVEN FROM THOSE LOW POSITIONS. THESE ARE SERIOUS CLAIMS AND WILL WARRANT ADDITIONAL INVESTIGATION AND EVALUATION.

MADAM CHAIRMAN, I AM PLEASED TO HAVE SUPPORTED THE MAGNETIC FUSION PROGRAM AND AM PROUD OF THE MANY ACCOMPLISHMENTS I HAVE SEEN WHILE ON THIS COMMITTEE. I ONLY HOPE THAT THERE ARE NOT IMPORTANT SEGMENTS OF OUR SOCIETY BEING EXCLUDED FROM PARTICIPATION AND CONTRIBUTION.

THANK YOU, MADAM CHAIRMAN.

Mr. **Walker**. Thank you, Madam Chairman.

Dr. Trivelpiece, I said to the staff up here as the slide was on showing the tritium facilities that I've got about 600,000 gallons of it sitting up at Three Mile Island that we would like to give away. Do you know anybody that might be interested?

Dr. **Trivelpiece**. Not offhand, but I'll look into it for you.

Mr. **Walker**. OK. Thank you.

In your chart showing international cooperation in the magnetic fusion program, what is the extent of cost sharing among the several programs?

Dr. **Trivelpiece**. Cost sharing in what sense?

Do you mean what we put in versus other countries?

Mr. **Walker**. Exactly.

Dr. **Trivelpiece**. The normal procedure with respect to facilities and use of facilities in scientific research essentially is that the host country pays for the facility, its operation and so on. If guest scientists use it and require personal property in the way of measurement tools and apparatus and so on, they bring that with them. Generally speaking, the sending side pays. That is more or less the standard approach. There are other arrangements. One of those arrangements is that which is currently going on with the GA Technologies program on which the Japanese are doing research. They have paid, I think, something in the order of $70 million over the past 5 years and are currently utilizing that facility. They have contributed to some of the experimental apparatus that's there. Generally speaking, it is the sending side that pays, but in this case there has been actual financial contribution from the Japanese. By and large, there has not been an exchange of funds with respect to other international programs. There has been an exchange of scientists who have used the facilities at the host country.

Mr. **Walker**. In terms of money it's a fairly insignificant amount, is that fair to say?

Dr. **Trivelpiece**. I would say it's a significant amount given the character of international cooperation, but in terms of the absolute size of the U.S. fusion program over 5 years it's insignificant.

Mr. **Walker**. To pursue the question that the chairman raised a moment ago I would like to go back to the testimony that we heard yesterday from both the industry and from the university. I gathered it centers on the idea that they feel they are being shut out in some of this research work. I would just appreciate your comment on that. It has some disturbing connotations because it seems to me that if we do have the feelings out there that they are being kept out of the process that the totality of our effort may be undermined.

Dr. **Trivelpiece**. Not just in the fusion program but with respect to many activities which the Department of Energy manages, people in the university community have felt somewhat excluded. The question is whether they are, in fact, excluded or whether it is a matter of perception. In order to try to explore this problem I was instrumental in instigating that the Department would have its Research Advisory Board convene a special panel to review all the Department's university programs. Dr. Ivan Bennett, who is a member of the Energy Research Advisory Board, is the chairman of the subpanel and he has been having groups in from universities to tell the Department what it thinks about how we go about doing our business with them. Now, the trouble in trying to deal with universities is that they are a diffuse and diverse community. If I talk about trying to deal with the national laboratories, there are maybe 10 or 15 people, we can get them all in a room at one time; we can talk to them and understand what their problems are. The university community consists literally of thousands of institutions spread out all over the United States. To deal with them in a coherent way is a little bit more difficult. It was much the same concern that I had with respect to the Department's fusion program and its interaction with the universities that stimulated having the Magnetic Fusion Advisory Committee, which Dr. Davidson will talk about, conduct a review of the fusion programs' university activities in order to see if we are utilizing universities in the most appropriate manner; are we receiving the best advice; are we funding them in the best fashion in order to take advantage of the skills and talents that they have. That review is in progress. Without presupposing the answer I can only say that we are interested in hearing what they have to say. I don't believe it turns out that we are not taking their advice into account. They are listened to and have been throughout the course of this program. Whether someone is listened to or whether or not you act smartly in respect to their advice is a different thing. It may be that we are not responding in a manner that they would regard as appropriate. I do believe that we do have adequate channels by which to receive the concerns that they express.

Mr. **Walker**. I guess my concern is that we heard it almost unanimously from both industry and from universities and they claim that there was no collusion there, that they were not trying to make a singular statement, that that was their individual experiences with it. From my standpoint, the danger is in what you point out as a strength, that you can talk to the national laboratories, you can get them together, you can find out what the problems are, you can basically move them in certain directions. That is a strength of the program. It also becomes a weakness of the program in that it's administratively fairly easy to handle, to target the efforts in that direction. The end result is that a lot of industries and universities then feel as though they've been shoved aside for the more direct administrative route. That's where it seems to be a strength and ends up becoming a weakness in the gathering of research data.

Dr. **Trivelpiece**. This is a mission oriented program and the Department has the responsibility for trying to produce progress with respect to certain goals and essentially is charged with doing this in the most efficient and effective manner. We have to use the tools that are available to us in accomplishing that. To the extent that universities as a collective institution can play a role in that they should. But we should not alter the rate of progress in a substantial manner simply to be aware of the institutional problems that the universities have in this area. It is very clear that without them there would not be the trained manpower. Many of the good ideas that the fusion program is exploring that have come from universities, but they are not a substitute for a national laboratory in terms of their ability to put together resources in a coordinated coherent way to do the kinds of large experimental programs that are now recognized as being the appropriate way to make progress in fusion.

Mr. **Walker**. Thank you, Madam Chairman.

Mrs. **Bouquard**. Thank you, Mr. Walker.

Mr. Winn?

Mr. **Winn**. Thank you, Madam Chairman. I think both the chairman and Mr. Walker touched on the point that concerns practically all of us. Whether it be industry or the university environment it seems to be that there's sort of a vague operation there. Maybe you don't see it that way, but that's the report that we got yesterday. For instance, we had a hearing which consisted of two panels and the first panel was five members of industry. Even though they said that they had not gotten their heads together nor their act together, the message was all the same. That was that industry is not participating in the magnetic fusion program to the extent that they think that they can or they should. I think that's our point and you made your answer. But in your testimony which you submitted in advance you claimed that the fusion program does have substantial industrial participation with about, I think you said, 30 percent direct funding to the industry. My question, Doctor, is this: How come, if that's the case, we get the type of comments and remarks from the industry Panel that we did yesterday?

Dr. **Trivelpiece**. I think there is a certain degree of enthusiasm on the part of industrial corporations to become more heavily involved in the fusion program. But from a historical point of view you have to remember that the activities that have brought us to the present stage of success in the fusion program involved research that has taken place over a 20-to-30-year period and that had its origins in the national laboratories. Those have grown to the point that we believe we are in striking distance of demonstrating scientific feasibility. At this stage it is appropriate to try to have a strategy by which you can get industry involved because ultimately it is going to be industry that will build fusion reactors. It would be inappropriate to say shut down half of the work at the laboratory and have engineering effort begin at some industries in order to cause industrial involvement to take place and at the same time cause scientific progress in the program to grind to a halt. So, it's a matter of balance. I think it's an issue in balance at the margin of where and how rapidly do you bring industry in. But it is true, as you indicated, something like a third of the Department's funds for fusion do go either directly or in a passthrough mode into industry. The coils for the large coil project are certainly not being built by physicists. They are being built by industrial organizations. It's a matter of balance and it's a matter of timing. I would disagree with the idea that industries inputs are not received. The Magnetic Fusion Advisory Committee provides a systematic process through a federally constituted advisory committee, by which these concerns can be brought to the Department in a formal manner and which we're certainly obliged not to ignore. We may not necessarily take the advice but we are certainly obliged to receive it and at least comment on it.

This particular debate has not been raised in a way that would cause the Magnetic Fusion Advisory Committee to bring forward to the Department any recommendations for doing things differently than we are now. If that needs to be addressed to them I would certainly be delighted to do so and will look into this after the hearing.

Mr. Winn. That was going to be my next suggestion. That maybe there is something lacking there as far as communication or what they think their part on the team might be. I'm sure that you are not going to adopt everything that they would like to have put into the program. That's understandable.

Educate me a little bit, if you will, about this business of participation. If industry is a member of a team at a national laboratory is all of the work being done at the laboratory?

Dr. Trivelpiece. No. I would use the large-coil project as an example.

Mr. Winn. That's kind of a special technology.

Dr. Trivelpiece. You mean the facility that's located at Oak Ridge?

Mr. Walker. Yes.

Dr. Trivelpiece. The coils are being built by three separate U.S. corporations as well as three corporations in three other countries. That certainly is providing a degree of participation in that particular area of technology.

The Westinghouse Corp., for years, has had some of its engineers and scientists on site at the Princeton University Plasma Physics Laboratory, where they have worked right along with the scientists at Princeton that are supported by the Government. Don Grove who was one of those employees, was one of the leaders in bringing the TFTR on line. You can find a number of other examples similar to that in which there are individuals from corporations that have both been supported by the Government and supported by a corporation who are working at the laboratories. I think there is a desire on our part to enhance this. It is a question of balance within a constrained resource. It's a question as to where you draw the line and how rapidly you bring things on.

There is a problem of frustrated expectations in perhaps moving too fast. I think it is wrong to bring industry in for a few months and then send them out and then bring them in for a few more months and send them out. That doesn't do anybody any good. We have some experience with this in several DOE programs in which various technologies, at considerable expense to the Government and considerable expense to the corporations, have been brought to a point of high advancement only to find then that a 2- or 3-year peroid goes by in which that particular technology is not needed for the program to make progress in its research. The individuals at the company get broken up and the people who were skilled in those arts are dissipated within the company or move on to other activities. Then we have to start over again. Therefore, it's a matter of bringing them on in a mode which permits a sustained involvement as distinct from a transient involvement, which I don't think does the corporations any good.

Mr. Winn. From your description it sounds like you're talking about the suppliers. The idea I got in the hearings yesterday was that industry wants to participate in the planning, management, basic design, and things of that sort.

Dr. TRIVELPIECE. We certainly welcome their advice.

Mr. WINN. Through the advisory council do they have the right to do that?

Dr. TRIVELPIECE. I believe that they do.

Mr. WINN. You think they do. How come they think they don't? That's my question.

Dr. TRIVELPIECE. There's some difference of opinion in the sense that as the agency of the Government that is responsible for the fusion program, we can't abrogate the responsibility to manage the program. We can take advice from many quarters but we can't turn over to industry the responsibility to do that which has been assigned to the Government.

Mrs. BOUQUARD. That's the reason you have the Fusion Design Center.

Dr. TRIVELPIECE. I was going to mention that at Oak Ridge there is a fusion engineering design center in which several industries competitively sought to participate and several are now participating. That does involve activities of planning and design with respect to future devices.

Mr. WINN. Let me ask one more question, if I might.

When you've got the labs here and you've got suppliers and other industry people out around the edges furnishing certain parts or doing certain things, maybe that system is part of the problem that they are referring to. There's a lack of communication. They seem to think that there is and you think it is all in place for that communication to take place; we obviously have a difference of opinion. I'm not taking one side or the other. I'm trying to see to it that we improve the situation.

Dr. TRIVELPIECE. It certainly would be desirable to improve it. I don't deny that we would probably be in a better position if we could more directly involve a higher level of industrial activity, but then it's back to the balance of resources that are available at the moment to run the program.

Mr. WINN. Even though we are talking about industry why don't you give it the old college try?

Thank you, Madam Chairman.

Mrs. BOUQUARD. Thank you, Mr. Winn.

One final comment, Doctor. Do you think there might be a little bit more industrial participation if the funding were not quite so slim. They are pretty well limited from a budget standpoint as to what they can do right now.

Dr. TRIVELPIECE. That's an interesting question, but as a representative of the administration I think it's not my place to make a suggestion as to whether or not additional funds would be appropriately spent in that manner.

Mrs. BOUQUARD. Thank you very much.

Dr. TRIVELPIECE. I would now like to ask Dr. Davidson to join me.

Mrs. BOUQUARD. Dr. Davidson, we have your complete testimony and we thank you for your summary. You may proceed as you wish.

Dr. TRIVELPIECE. May I add a comment?

Mrs. BOUQUARD. Yes, sir.

Dr. **Trivelpiece.** I would like to precede Dr. Davidson with just a few brief remarks.

It was about a year ago that I recognized the need to try to improve the approach to receiving information from universities and from industry and from the scientists and engineers who were in the fusion program.

I receive a lot of advice. It usually comes in the form of you should have done something. Or you should not have done something. Quite frequently it is is the same object which one group is saying you should have done that the other group is saying you should not have done. In an attempt to try to provide a regular and systematic forum in which these things could be debated before they came to the Department's official attention, I asked Dr. Davidson together with a collection of his colleagues from industry and universities and national laboratories to serve as this Magnetic Fusion Advisory Committee. It is a formally constituted advisory committee. It meets in public. There were a collection of questions that we put to them last year and he will talk about the answers and the studies that they have done in that respect. I do want to go on record at this point as expressing very sincere appreciation, not only to Dr. Davidson, but to a large collection of people who spent many hours from last July, when this committee was first convened, in bringing about some very thoughtful reports and excellent analysis. As an advisory committee, I think, they have done an absolutely first rate job.

Whether we will agree with all the things they bring forth or not will remain to be seen. But I certainly would like to take this occasion to compliment them and express our appreciation for a job well done.

Mrs. **Bouquard.** Thank you. I'm sure that Dr. Davidson appreciates that introduction as much as we do.

STATEMENT OF DR. RONALD C. DAVIDSON, DIRECTOR, PLASMA FUSION CENTER, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, AND CHAIRMAN, DEPARTMENT OF ENERGY, MAGNETIC FUSION ADVISORY COMMITTEE

Dr. **Davidson.** Madam Chairman, I am pleased to have this opportunity to address the subcommittee on behalf of the Magnetic Fusion Advisory Committee, which was formed in June 1982. MFAC provides advice and guidance on a continuing basis to the Secretary of Energy through the Director of Energy Research on the magnetic fusion program.

The present membership of MFAC is listed on the first Vu-Graph. As you can see there is a broad institutional representation on this committee from industry, universities, and the national laboratories. Also the committee membership has strong technical expertise in a wide range of engineering and scientific areas.

To briefly summarize procedures, MFAC meets approximately four times per year in public session. We receive written charges from the DOE Director of Energy Research, Dr. Trivelpiece. MFAC establishes a panel of experts in each area of evaluation and supplements the DOE charges with additional guidelines, where appropriate. These panels, which report to MFAC, collect information, prepare findings and recommendations, and meet as often as required to formulate a final report to MFAC within reasonable time constraints.

The panel findings and recommendations are then reviewed in public session by the full Magnetic Fusion Advisory Committee. Based on the panel input and extensive committee deliberations, MFAC then develops and forwards recommendations to DOE in each charge area.

In the material submitted for the record, I have included a detailed compendium of the DOE charges to MFAC since June 1982, the final MFAC findings and recommendations to DOE in those charge areas where our evaluation has been completed, and information on the new charge areas under consideration at the present time.

The next Vu-Graph gives an outline of the charge areas and chronology.

At our first meeting in June 1982, we receive three charges from the Department of Energy. To summarize very briefly, in charge area No. 1 we were asked to review and comment on the makeup and pace of the tandem mirror and the tokamak programs as they contribute to the scientific and technological base needed to evaluate the possible consideration as candidates for an experimental test reactor.

In charge area No. 2, we were asked to review the stellarator, Elmo Bumpy Torus, and reversed field pinch concepts and to advise the Department of our views on their relatively priority as backups to the mainline tokamak and tandem mirror reactor development programs.

We were also asked to formulate recommendations in the context of three hypothetical budget cases: $600 million, $500 million, and $400 million view as constant effort levels in fiscal year 1984 and beyond.

Later in this testimony, I will refer to these three budget cases as enhanced—corresponding to $600 million—constrained—corresponding to $500 million—and reduced—corresponding to $400 million.

In the first two charge areas listed on the Vu-Graph, it is estimated that approximately 140 man-weeks of activity were involved in formulating the panel reports and recommendations to MFAC. After extensive deliberations, the Magnetic Fusion Advisory Committee submitted its findings and recommendations to DOE in November 1982.

In charge area No. 3, we were asked to evaluate TFTR upgrade options and possible programmatic roles of the TFTR facility after feasibility experiments. The MFAC findings and recommendations on TFTR uptrades options and the related report by panel No. 3 were transmitter to DOE in January 1983.

Also in January, we received two new charges from the Department, listed on the Vu-Graph. Charge No. 4 relates to an evaluation of MFTF-B upgrade options. Charge No. 5 relates to an evaluation of the long-term role of universities in the fusion program. Expert panels have been established in each of these areas, and we expect to receive panel recommendations at our May and summer meetings, respectively.

I will now highlight selected MFAC findings and recommendations in the first three charge areas. As indicated earlier a complete compendium has been submitted for the record.

As a general remark, the Magnetic Fusion Advisory Committee has been impressed by the very significant recent technical advances in the magnetic fusion program. Without enumerating the specific advances here, one of our principal findings is that there continues to be notably broad and vigorous technical progress in the consolidation of the scientific and technological data base for magnetic fusion. At its meeting in November, MFAC recommended that DOE take steps in the budget process to restore program momentum and to return to an accelerated course of development consistent with the significant technical progress. It was recommended that this should be at the program-driven or enhanced level.

I will now highlight a few of the principal MFAC findings and recommendations with regard to tandem mirrors and tokamaks.

At present, the tandem mirror and tokamak concepts can be embodied in viable reactor designs of roughly similar characteristics. In both reactor designs, there are scientific and technological assumptions that remain to be validated by experiment. Both designs also have the potential for significant further improvement through technical innovations that are in the exploratory phase.

In the case of the tokamak, the existing data base implies the feasibility of net power production. Demonstration of the achievability of sufficiently high power density and long pulse length for commercial attractiveness is being addressed by the experimental program. Steady-state current drive at low recirculating power is a promising option for concept improvement.

Tandem mirror research is at an earlier phase of development. The current experimental program is addressing the feasibility of net power production. In this context, the demonstration of the thermal barrier technique on TMX-upgrades is of particular importance. The tandem mirror concept offers the possibility of driven steady-state, high-beta operation.

The program plans that were developed by MFAC panel No. 1, in the enhanced case, that is to say the $600 million case, in the opinion of MFAC have high technical merit both with regard to program content and prospects for establishing the tandem mirror and tokamak data base required for initiation of ETR construction at the end of 1987.

In the constrained case, which is the nominal $500 million case, MFAC concluded that the program plans developed by panel No. 1 maintain a logic similar to the enhance case, with focus on critical physics and technology issues but a stretchout by several years of the preparation phase for an engineering test reactor. Actual implementation of the ETR would require incremental funding above the $500 million level. The constrained case, in our opinion is marked by a significant increase in technical risk in establishing the necessary data base for an early ETR decision.

I will now highlight several of the principal findings and recommendations with regard to the backup concepts consisting of the Elmo Bumpy Torus, reversed field pinch, and stellarator approaches to magnetic confinement. Considerably more detail can be found in the written statement.

Each of these confinement approaches, while at an earlier stage of scientific development, is characterized by a rapidly evolving and encouraging data base. Each concept, to varying degrees, also offers potential reactor advantages, be it high power density, as in the reversed field pinch, steady-state operation, as in the EBT or stellarator concepts, high modularity, as in the EBT concept, et cetera, depending on the concept.

It is MFAC's assessment that the research in the backup concepts has resulted in significant advances in the basic physics understanding of plasma confinement properties. Moreover, the technology development and innovative reactive features associated with the backup concepts have had a favorable impact on the development of the mainline approaches. It was our conclusion that even at a reduced budget level, it is important that at least the present fraction of the confinement effort be devoted to experimentation on backup concepts, not motivated by mainline ETR considerations. The physics and technology payoff of such independent programs is very large.

It is also our recommendation that program emphasis should be placed on the development of backup concepts that show significant promise of an improved reactor product as well as a strong scientific data base.

Finally, assuming strong technical progress, further development of the backup concepts is recommended in both the enhanced and constrained cases. The pace of development should be based on technical merit and the availability of funds.

In the area of review related to charge No. 3, the Magnetic Fusion Advisory Committee met on January 10 and 11, and reviewed the findings and recommendations of MFAC panel No. 3 evaluating various TFTR upgrade options. The final panel report and summary of panel findings and recommendations were accepted in principle by MFAC and transmitted to Dr. Trivelpiece on January 11, 1983.

Let me summarize a few selected panel findings and recommendations presented to MFAC.

First of all, panel No. 3 concluded that the TFTR facility constitutes a unique resource in the U.S. fusion program, and full advantage should be taken of this asset. Cost-effective improvements to TFTR, both in the short-run and long-run are to be encouraged.

Panel No. 3 also concluded that among the options considered at the time of the review, the most attractive, from a technical standpoint, was the DCT-8 option proposed by Princeton, consisting of a superconducting D-T burning device capable of ignition and very long-pulse operation. This option would permit a thorough characterization of the plasma core of a fusion reactor and would incorporate many reactor prototypical engineering features.

To summarize the findings and recommendations of MFAC in transmittal of the panel 3 report to DOE, we agreed that the near-term demonstration of ignition and long-pulse equilibrium burn constitutes the technically necessary next major step for tokamaks beyond the TFTR break-even demonstration.

The committee also notes that such a step represents an evolution in the strategy of the tokamak program plan defined by panel No. 1. The mission of TFTR upgrade is raised to the level of the tokamak fusion core demonstration. The scope of the tokamak engineering test reactor, as the last step before commercialization, can be enhanced correspondingly.

In order to provide a reactor-relevant configuration on the road to a tokamak ETR, MFAC agreed with panel No. 3, that a superconducting-coil device such as DCT-8 is highly desirable. However, MFAC also recommends that lower-cost options for the achievement of ignition and long-pulse burn using copper coils, should continue to be investigated. The technical merit of copper-coil options relative to superconducting-coil options should be determined in the light of budgetary constraints and technological needs.

While the construction costs for an equilibrium burn experiment of the DCT-8 type are substantially smaller than those for the FED or a tokamak ETR, MFAC also agrees with panel No. 3 that incremental funding above the base program level would be required for timely implementation.

Finally, in our upcoming meeting in May we will hear from Panel No. 4 regarding their evaluation of MFTF-B upgrade options. Following that review, we have suggested to the Department that MFAC integrate and prioritize the findings and recommendations regarding tokamaks and tandem mirrors, that's charge area No. 1, alternate fusion concepts, charge area No. 2, TFTR upgrades options, charge area No. 3, and MFTF-B upgrade options, charge area No. 4, and that we do this within the context of the overall fusion program strategy and projected fusion budgets.

Thank you.

[The prepared statement of Dr. Davidson follows:]

SUMMARY OF
MAGNETIC FUSION ADVISORY COMMITTEE
FINDINGS AND RECOMMENDATIONS[†]

Presented by
Ronald C. Davidson
Chairman, Magnetic Fusion Advisory Committee

Presented to
THE SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
WASHINGTON, D.C., 20515

March 15, 1983

[†] Oral Testimony

Madam Chairman, I am pleased to have this opportunity to address this Subcommittee on behalf of the Magnetic Fusion Advisory Committee, which was formed in June, 1982. The Magnetic Fusion Advisory Committee provides advice and guidance on a continuing basis to the Secretary of Energy through the Director of Energy Research on the Magnetic Fusion Energy Program. This advice and guidance includes (Vugraph #1):

- Periodic reviews of elements of the program and recommendation of any changes considered desirable on the basis of scientific and technological advances or other factors.
- Advice on long-range plans, priorities, and strategies to demonstrate the scientific and engineering feasibility of fusion.
- Advice on recommended appropriate levels of funding to develop those strategies and to help maintain appropriate balance between competing elements of the program.
- Advice and guidance on any issues relating to the program as requested by the Secretary or the Director of Energy Research.

The present membership of MFAC is listed on the second vugraph. As you can see, there is broad institutional representation on this committee from industry, universities, and the national laboratories. Also the committee membership has strong technical expertise in a wide range of engineering and scientific areas. It is a pleasure to serve with such a distinguished and dedicated group.

To briefly summarize procedures, MFAC meets approximately four times per year in public session. We receive written charges from the DOE Director of Energy Research, Dr. Trivelpiece. MFAC establishes a panel of experts in each area of evaluation and supplements the DOE charges with additional guidelines, where appropriate. The composition of a typical panel includes several members of MFAC and many additional panel members drawn from the technical community. These panels, which report to MFAC, collect information, prepare findings and recommendations, and meet as often as required to formulate a final report to MFAC within reasonable time constraints.

The panel findings and recommendations are then reviewed in public session by the full Magnetic Fusion Advisory Committee. Based on the panel input and extensive committee deliberations, MFAC then develops and forwards recommendations to DOE in each charge area.

In the material submitted for the record, I have included a detailed compendium of the DOE charges to MFAC since June, 1982, the MFAC guidelines to the various panels, the final MFAC findings and recommendations to DOE in those charge areas where our evaluation has been completed, and information on the new charge areas under consideration at the present time.

The third vugraph gives an outline of the charge areas and chronology.

At our first meeting in June, 1982, we received three charges from DOE. To summarize very briefly, in charge area #1 we were asked to review and comment on the makeup and pace of the tandem mirror and the tokamak programs as they contribute to the scientific and technological base needed to evaluate their possible consideration as candidates for an Experimental Test Reactor (ETR) Project within the next decade.

In charge area #2, we were asked to review the Stellarator, Elmo Bumpy Torus, and Reversed Field Pinch concepts and to advise DOE of our views on their relative priority as backups to the mainline tokamak and tandem mirror reactor development programs.

We were also asked to formulate recommendations in the context of three hypothetical budget cases: $600M, $500M, and $400M, viewed as constant effort levels in FY84 and beyond.

Later in this testimony, I will refer to these three budget cases as "enhanced" ($600M), "constrained" ($500M), and "reduced" ($400M).

In the first two charge areas alone it is estimated that approximately 140 man-weeks of activity were involved in formulating the panel reports and recommendations to MFAC. After extensive deliberations, the Magnetic Fusion Advisory Committee submitted its findings and recommendations to DOE in November, 1982.

In charge area #3, we were asked to evaluate TFTR upgrade options and possible programmatic roles of the TFTR facility after feasibility experiments. The MFAC findings and recommendations on TFTR upgrade options and the related report by Panel #3 were transmitted to DOE in January, 1983.

Also in January, we received two new charges from DOE. Charge #4 relates to an evaluation of MFTF-B upgrade options. Charge #5 relates to an evaluation of the long-term role of universities in the fusion program. Expert panels have been established in each of these areas, and we expect to receive panel recommendations at our May and summer meetings, respectively.

I will now highlight selected MFAC findings and recommendations in the first three charge areas. As indicated earlier, a complete compendium has been submitted for the record.

The Magnetic Fusion Advisory Committee was impressed by the very significant recent technical advances in the magnetic fusion program. Without enumerating the specific advances here, one of the principal findings of MFAC is that there continues to be notably broad and vigorous technical progress in the consolidation of the scientific and technological data base for magnetic fusion. The U.S. magnetic fusion program has reached its present state of world leadership by capitalizing on a series of scientific successes, with new facilities following up promising technical developments, while discarding less promising concepts. This policy has produced a balanced program directed at: (a) the world's first D-T demonstration of scientific feasibility, expected in the Tokamak Fusion Test Reactor, (b) facilities, including the Mirror Fusion Test Facility, partly constructed, that will allow a comparative evaluation of the two main variants of magnetic confinement geometries (closed and open systems), (c) continued exploration of alternative concepts, and (d) test facilities exploring critical technology issues such as superconducting magnets and the handling of tritium.

It is this broad-based but tightly integrated research program that provided the basis for an aggressive pursuit of the engineering development of fusion envisioned in the Magnetic Fusion Engineering Act of 1980. However, while foreign programs, especially in Japan, continue at a vigorous pace, budget pressures have begun to erode the U.S. program, causing stretchout of research on the mainline approaches, lengthy delays in facilities for alternative concepts and for advanced technologies, and elimination of engineering development activities envisioned at the Center for Fusion Engineering. The Magnetic Fusion Advisory Committee recommends that DOE take immediate steps in the FY84 budget process to restore program momentum and to return to an accelerated course of development consistent with the significant technical progress. This should be at least the "program-driven" or "enhanced" level.

Tandem Mirrors and Tokamaks

I will now highlight several of the principal MFAC findings and recommendations with regard to tandem mirrors and tokamaks.

At present, the tandem mirror and tokamak concepts can be embodied in viable reactor designs of roughly similar characteristics. In both reactor designs, there are scientific and technological assumptions that remain to be validated by experiment.

Both designs also have the potential for significant further improvement through technical innovations that are in the exploratory phase.

In the case of the tokamak, the existing data base implies the feasibility of net power production. Demonstration of the achievability of sufficiently high power density and long pulse length for commercial attractiveness is being addressed by the experimental program. Steady-state current drive at low recirculating power is a promising option for concept improvement.

Tandem mirror research is at an earlier phase of development. The current experimental program is addressing the feasibility of net power production. In this context, the demonstration of the thermal barrier technique on TMX-Upgrade is of particular importance. The tandem mirror concept offers the possiblity of driven steady-state, high-beta operation.

The TFTR facility is expected to demonstrate the scientific feasibility of magnetic fusion during the mid 1980s. In order to continue high-performance tokamak experimental progress during the first part of the 1990s, an appropriate upgrade of the TFTR facility should be considered for operation in this period. This topic has been addressed by MFAC and I will report our findings later in this presentation.

The development of the required data base for the tandem mirror reactor concept should be pursued vigorously. Essential to this is the timely completion of the MFTF-B facility now under construction. Future upgrade options for MFTF-B are now being evaluated by MFAC Panel #4.

The development of tandem mirrors and tokamaks has greatly benefited from innovative research on medium- and small-scale experimental facilities, and vigorous support activities in development and technology, plasma theory, and computations. Continued high priority should be given to these activities.

The program plans developed by MFAC Panel #1 in the "enhanced"[†] (nominal $600M) case have high technical merit both with regard to program content and prospects for establishing the tandem mirror and tokamak data base required for initiation of ETR construction at the end of 1987.

In the "constrained" (nominal $500M) case, the program plans developed by MFAC Panel #1 maintain an internal logic similar to the "enhanced" (nominal $600M) case, with focus on critical physics and technology issues but a concomitant stretch-out by several years of the preparation phase for ETR. Actual implementation of the ETR would require incremental funding. The "constrained" case is marked by a significant increase in technical risk in establishing the necessary data base for an early ETR decision.

In the "reduced" (nominal $400M) case, the program plans have not been formulated and analyzed in sufficient detail to permit specific recommendations by MFAC at this time. However, it is evident that the "reduced" budget case would have a major negative impact on overall program pace and U.S. leadership in fusion energy development, and would require a dismantling of key elements of the present goal-oriented program.

[†] Here, the "enhanced" program refers to the nominal "$600M" case discussed by Panel #1. In this regard, the "enhanced" budget total in the "out" years (beyond FY84) exceeds $600M, depending on the pace of major projects.

Elmo Bumpy Tori, Reversed Field Pinches and Stellarators

I will now highlight several of the principal MFAC findings and recommendations with regard to the backup concepts consisting of the Elmo Bumpy Torus, Reversed Field Pinch, and Stellarator approaches to magnetic confinement. Considerably more detail can be found in the written statement.

Each of these confinement approaches, while at an earlier stage of scientific development, is characterized by a rapidly evolving and encouraging data base. Each concept, to varying degrees, also offers potential reactor advantages, be it high power density, steady-state operation, high modularity, etc., depending on the concept.

The reversed field pinch has the potential to produce a qualitatively different reactor product for the fusion program, namely a high-power-density, high-beta, compact reactor with prospects for ignition solely with ohmic heating. Demonstration of sustained, stable operation at high beta and favorable confinement scaling at high current are essential requirements for achieving this reactor potential. Large extrapolations of confinement behavior are required in considering reactor potential.

The conventional stellarator has the most advanced scientific data base of all backup concepts. Most stellarator reactor designs lead to a product qualitatively similar to those of the mainline approaches, with the potential advantages of low recirculating power and steady-state, disruption-free operation. Recent advanced stellarator concepts offer the prospect of high beta and high power density.

The Elmo Bumpy Torus is based on physics principles relating to transport that are different from those in the mainline. It offers a product qualitatively similar to those in the mainline, namely, a low-to-moderate power density, large-scale power reactor, but with the potential advantages of steady-state and high modularity. The realization of this potential requires large extrapolations of the behavior observed in the present EBT-S experiments.

It is MFAC's assessment that research on the backup concepts has resulted in significant advances in the basic physics understanding of plasma confinement properties. Moreover, the technology development and innovative reactor features associated with the backup concepts have had a favorable impact on the development of the mainline approaches. Even at the "reduced" (nominal $400M) budget level, it is important that at least the present fraction of the confinement effort be devoted to experimentation on backup concepts not motivated by mainline ETR considerations. The physics and technology payoff of such independent programs is large.

It is also our recommendation that program emphasis should be placed on the development of backup concepts that show significant promise of an improved reactor product as well as a strong scientific data base.

In the "reduced" (nominal $400M) case, MFAC concludes that reduced budgets do not permit advancement of any of the backup concepts to the proof-of-principle level. It is clear that the "reduced" budget case would have a large negative impact on development of the backup concepts. Nonetheless, exploratory research programs should be maintained on existing facilities (e.g., EBT-S and ZT-40) with particular emphasis on innovative concepts and improved understanding of confinement properties. Stellarator research should continue at the "study" level, with the possibility of undertaking modest experiments.

Finally, assuming continued strong technical progress, further development of the backup concepts is recommended in the "enhanced" (nominal $600M) and "constrained" (nominal $500M) cases. The pace of development should be based on technical merit and availability of funds.

TFTR Upgrade Options

The Magnetic Fusion Advisory met on January 10 and 11, 1983, and reviewed the findings and recommendations of the MFAC Panel #3 evaluating various TFTR upgrade options. The final Panel report and summary of Panel findings and recommendations were accepted in principle by MFAC and transmitted to Dr. Trivelpiece on January 11.

Let me summarize a few selected Panel findings and recommendations presented to MFAC.

First, Panel #3 concluded that the TFTR facility constitutes a unique resource in the U.S. fusion program, and full advantage should be taken of this asset. Cost-effective improvements to TFTR, both short-run and long-run, are to be encouraged.

Moreover, Panel #3 concluded that the next important steps in the tokamak program are to demonstrate:

- Plasma ignition and equilibrium burn control.
- Reactor-relevant engineering configuration.
- Long-pulse or quasi-steady-state operation.

Accomplishing all of these steps in a single, near-term device would require incremental funding beginning in FY85 and beyond to at least the enhanced ($600M) budget case.

Panel #3 also concluded that among the options considered, the most attractive from a technical standpoint is the PPPL proposed option (termed here "DCT-8") consisting of a superconducting D-T burning device capable of ignition and very long-pulse operation. The DCT-8 option would permit a thorough characterization of the plasma core of a fusion reactor and would incorporate many reactor prototypical engineering features. Moreover, it would provide renewed impetus to fusion research and would facilitate a more aggressive community stance with respect to tokamak ETR objectives.

Finally, industrial involvement and integration into the program should be undertaken wherever and whenever possible if the industries are to be prepared to make meaningful contributions in the ETR era and beyond. Such participation in all TFTR upgrade options should be factored into the program plan.

To summarize MFAC findings and recommendations in the transmittal of the Panel #3 report to DOE, the Magnetic Fusion Advisory Committee agrees that the near-term demonstration of ignition and long-pulse equilibrium burn constitutes the technically necessary next major step for tokamaks beyond the TFTR break-even demonstration.

The Committee also notes that such a step represents an evolution in the strategy of the tokamak program plan defined by Panel #1. The mission of the TFTR Upgrade is raised to the level of a Tokamak Fusion Core Demonstration (TFCD). The scope of the tokamak Engineering Test Reactor (ETR), as the last step before commercialization, can be enhanced correspondingly.

In order to provide a reactor-relevant configuration on the road to a tokamak ETR, a superconducting-coil device such as DCT-8 is highly desirable. However, MFAC also recommends that lower-cost options for the achievement of ignition and long-pulse burn, using copper coils, should continue to be investigated. The technical merit of copper-coil options relative to superconducting-coil options should be determined in the light of budgetary constraints and technological needs.

While the construction costs for an equilibrium burn experiment of the DCT-8 type are substantially smaller than those for the FED or for a tokamak ETR, MFAC agrees with Panel #3 that incremental funding above the base program level will be required for timely implementation.

Finally, in our upcoming MFAC meeting in May, we will hear from Panel #4 regarding their evaluation of MFTF-B upgrade options. Following that review, we have recommended to DOE that MFAC integrate and prioritize the findings and recommendations regarding tokamaks and tandem mirrors (Charge #1), alternate fusion concepts (Charge #2), TFTR upgrade options (Charge #3), and MFTF-B upgrade options (Charge #4) within the context of the overall fusion program strategy and projected fusion budgets.

MAGNETIC FUSION ADVISORY COMMITTEE

CHARTERED AREAS OF ADVICE AND GUIDANCE TO DOE

- Periodic reviews of elements of the program and recommendation of any changes considered desirable on the basis of scientific and technological advances or other factors

- Advice on long-range plans, priorities, and strategies to demonstrate the scientific and engineering feasibility of fusion

- Advice on recommended appropriate levels of funding to develop those strategies and to help maintain appropriate balance between competing elements of the program

- Advice and guidance on any issues relating to the program as requested by the Secretary or the Director of Energy Research

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	AREA OF MFAC EVALUATION	PANEL CHAIRPERSON	DOE Charge Received	MFAC Recommendations to DOE
#1	Tandem Mirrors & Tokamaks	Dr. John Gilleland G.A. Technologies	June, 1982	November, 1982
#2	Alternate Confinement Concepts	Prof. Robert Conn UCLA	June, 1982	November, 1982
#3	TFTR Upgrade Options	Dr. Harold Forsen Bechtel	June, 1982	January, 1983
#4	MFTF-B Upgrade Options	Dr. John Gilleland G.A. Technologies	January, 1983	Pending
#5	Long-Term University Role	Prof. Robert Gross Columbia University	January, 1983	Pending

MAGNETIC FUSION ADVISORY COMMITTEE

REPORT ON

EVALUATION OF MAGNETIC CONFINEMENT CONCEPTS

Submitted to

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C., 20585

October, 1982

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1. INTRODUCTION AND BACKGROUND

At its first meeting on June 1 and 2, 1982, the Magnetic Fusion Advisory Committee (MFAC) was asked to review and make recommendations regarding the overall balance, priorities and program plans related to (a) the mainline tandem mirror and tokamak confinement approaches, and (b) the backup Elmo bumpy torus, reversed field pinch and stellarator confinement approaches. Specifically, the related requests excerpted from Dr. Alvin W. Trivelpiece's June 1 letter are (Appendix A):

1. "It now appears that only steady-state tokamaks and tandem mirror reactor concepts will be at similar stages of development with respect to their potential as reactor candidates by 1986-87. Please review and comment on the makeup and pace of the tandem mirror and the steady-state tokamak programs as they contribute to the scientific and technological base needed to evaluate their possible consideration as candidates for an Experimental Test Reactor (ETR) Project within the next decade. Also, please assess the completeness of the experimental facilities now planned to bring these concepts to such a comparison stage, and make recommendations as to the most complete program allowed by the above budget levels. In addition to establishing the scientific base for an ETR what priority should be given to technology development needed to establish a technical base for building an ETR prior to the ETR decision?"

2. "In addition to these two mainline reactor programs, some backup program should be continued on a longer time scale. Please review the Stellarator, Elmo Bumpy Torus, and Reversed Field Pinch concepts to advise us of your views on their relative priority as backups to the mainline steady-state tokamak and tandem mirror reactor development programs. In your deliberations and investigations of these concepts, please take the following into account:

    - Reactor characteristics
    - Scientific base for a Proof of Principle (POP) Experiment
    - Technology base for a POP Device
    - Development needs of a POP Device and the size and timing of a program to complete this development
    - Evaluation of international programs to provide similar information to that generated by a U.S. development program of the backup concept

- Nature and size of post-POP facilities needed to prepare a technical base for an ETR application

> Make a recommendation as to the desirability and choices of backup concepts in the above budget cases."

In addition, Dr. Trivelpiece's letter states:

> "For the various issues, the committee should consider three hypothetical budgets, ($400M-$500M-$600M), which for the foreseeable future should be regarded as constant effort levels. In qualitative terms, these could be regarded as declining, constant, and growing programs."

In response to these requests, the Magnetic Fusion Advisory Committee established two technical panels to review and make recommendations to MFAC regarding the priorities and program plans for (a) tandem mirrors and tokamaks (Panel #1), and (b) Elmo bumpy tori, reversed field pinches and stellarators (Panel #2). The panel membership and charges to the panels are summarized in Appendix A of this report. Moreover, the panel reports and recommendations are included in Appendix B (Panel #1) and Appendix C (Panel #2).

As further background information, the preparation of panel reports and recommendations were carried-out within the context of the following general guidelines (Appendix A):

"1. Review and make recommendations regarding the scientific and technological basis and program plans for each concept. Identify the strengths, weaknesses and primary characteristics of each confinement approach as a practical fusion reactor.

2. Delineate the nature and role of large-, medium- and small-scale facilities leading to an ETR decision point, and identify the supporting technology and applied physics activities necessary to implement program plans.

3. Address the role of international cooperation and technical information from fusion facilities in other nations in determining emphasis and priorities in the U.S. program.

4. Develop program plans and recommendations for two budget cases:

   (a) Flat budget case (in constant dollars), corresponding to $400M, $500M or $600M for the overall magnetic fusion program in FY 84 and beyond.

   (b) Enhanced budget case (but limited to an average real growth of 10% per year, say), corresponding to an accelerated ETR/FED decision point consistent with what is technically feasible.

To the maximum extent possible, realistic cost estimates are to be used in formulating program plans.

5. For those concepts that cannot compete for a particular ETR decision point, plans should be directed at concept optimization for the Fusion Demonstration Plant (FDP)."

In their extensive preparations and deliberations, the panels and subpanels had representation and technical input from a broad segment of the fusion community. In this regard, it is estimated that approximately 140 man-weeks of activity were involved in formulating the panel reports and recommendations.

The panel reports and recommendations form the substantive basis for the MFAC recommendations summarized in Sec. 2 of this report. While specific departures of MFAC recommendations from panel recommendations are delineated in Sec. 2, the Magnetic Fusion Advisory Committee endorses the overall sense of the panel findings. The Magnetic Fusion Advisory Committee takes this opportunity to commend the panels and subpanels for their conscientious effort in this important undertaking and for the high quality of their work.

With regard to Panel #1 and #2 reports (Appendices B and C) and the related recommendations by MFAC (Sec.2), it is important to recognize that many elements of the applied physics and development and technology programs were not evaluated in this process, except for those direct support areas identified by Panels #1 and #2. The absence of comment on these elements is a reflection of the specific charges of MFAC and not of a negative assessment of these programs.

Finally, at its first meeting on June 1 and 2, 1982, the Magnetic Fusion Advisory Committee was also asked to evaluate upgrade options for TFTR (see Dr. Trivelpiece's letter in Appendix A). A third technical panel (Panel #3) was established to review and make recommendations regarding TFTR upgrade options. Panel #3 has been actively addressing this subject during the past three months and will make specific recommendations to MFAC at its next (third) meeting. At its second meeting on August 30 and 31, the Magnetic Fusion Advisory Committee expanded the charge to Panel #3 to include an evaluation of MFTF-B upgrade options.

2. MAGNETIC FUSION ADVISORY COMMITTEE

FINDINGS AND RECOMMENDATIONS

A. Advances in Technical Data Base

The major near-term objective of the magnetic fusion program is to provide the scientific and technological data base for a commercially attractive fusion reactor concept. Summarized below are some of the major technical advances during the past two years for each of the confinement approaches evaluated by MFAC:

Tandem Mirrors

- End plugging by tandem mirror electrostatic potentials has been conclusively demonstrated on TMX with $n\tau_p = 10^{11}\mathrm{cm}^{-3}$ - sec in the central solenoid at temperatures $T_i \sim T_e \sim 100\mathrm{eV}$, representing two orders-of-magnitude improvement in confinement relative to a "single" mirror at the same temperature. Preliminary results on suppression of microinstabilities in TMX-Upgrade are encouraging.

- High-beta MHD stability in the central solenoid has been demonstrated with neutral beam heating in TMX (up to 20% average beta with anisotropic pressure, and 40% maximum beta on axis) and with ion cyclotron RF heating in Phaedrus. Experiments to test major advances in reactor end-plug design have been initiated ("axicell" magnet geometry on TARA and "thermal barriers" on TMX-Upgrade).

- MFTF-B construction has passed the midway point with successful tests of the first superconducting yin-yang (end-plug) magnet at full design field (7.7 tesla at the NbTi conductor).

Tokamaks

- Stable operation has been achieved in the ISX-B, PDX, and D-III tokamaks down to low MHD safety factors ($q < 2$). This technique has allowed confinement studies to be extended into the average stable beta range above 4%. The present outlook is favorable to achieving tokamak reactor objectives in the 5-10% beta range, even though some deterioration of confinement is observed at higher beta.

- RF heating at the ion and electron cyclotron frequencies has generated multi-keV ion and electron temperature increases in PLT and T-10 tokamaks, respectively. The measured efficiencies are high and point to economically attractive reactor heating systems. Recent neutral beam heating experiments on the ASDEX tokamak have identified operating regimes without degradation of confinement at high heating power.

- RF current drive in the lower-hybrid frequency range has been used to sustain tokamak currents of several hundred kiloamperes for several seconds in PLT plasmas of $10^{13}\mathrm{cm}^{-3}$ peak density, and for shorter times in Alcator C plasmas of $10^{14}\mathrm{cm}^{-3}$ peak density. The low-density results help insure the technical feasibility of the 10^4 second burn time desired for an economical tokamak reactor. The high-density results offer the promise of true steady-state tokamak operation at reactor-level densities.

Elmo Bumpy Tori

- Neoclassical electron transport scaling has been verified in 200kW, 28GHz operation of the EBT-S Elmo bumpy torus experiment. This increases confidence in projections for both EBT-P and the Elmo bumpy torus reactor, which are based on a neoclassical transport model.

- Important developments in coupled ring-core stability theory, and self-consistent application of the Elmo bumpy torus reactor model based on this theory predict attractive reactor performance, with a total recirculating power fraction of about 10%.

- Fast wave ion cyclotron heating experiments on EBT-S show efficient (over 40%) RF power coupling and ion heating. These experiments provide an important physics and technology base for testing ion confinement and stability on EBT-P.

Reversed Field Pinches

- Confinement experiments carried out on the ZT-40 reversed field pinch have produced plasma parameters that considerably exceed original expectations. ZT-40 is now operating at reactor-relevant beta levels up to 10-15 percent for 20 milliseconds. For plasma current in the 100-200 kiloampere range, energy confinement times of 0.2 - 0.3 milliseconds are observed on ZT-40 with fields of 1 - 2 kilogauss at the magnet coils. These confinement times are expected to increase with current as observed in the TPE-1R(M) reversed field pinch (Japan).

- The verification of a "dynamo" effect may simplify the reactor development of the reversed field pinch by providing a mechanism for sustaining the reversed-field configuration and also providing a potential mechanism for steady-state current drive.

- Advances in the systems analysis and development of reversed field pinch reactor models have altered the nature of reactor options available to the magnetic fusion program. The resistive-copper-magnet Compact-Reversed-Field-Pinch-Reactor (CRFPR), with its DT and DD fuel options, offers the prospect of an end product that is qualitatively different from conventional reactors in reducing the size and mass of reactor systems.

Stellarators

- Neutral-injection experiments in the Wendelstein VII A stellarator (Federal Republic of Germany) have achieved $n\tau_E = 3 \times 10^{12} \mathrm{cm}^{-3}\mathrm{sec}$ at ion temperatures $T_i = 1$ kev, corresponding to values of $n\tau_E$ that surpass all other non-tokamak devices.

- Current-free stellarator operation has been achieved both in neutral-injection experiments on W VIIA and in the afterglow of electron cyclotron heating on Heliotron E (Japan). In both cases, electron energy confinement in the current-free regime is substantially improved relative to tokamaks.

- Magnetically-trapped energetic beam ions, arising from perpendicular injection, are well confined in W VIIA, apparently due to the self-consistent radial electric field. Computational results including such fields are in agreement with the observations, and predict that ion energy confinement should be acceptable in a stellarator reactor. In W VII A, ion thermal transport is consistent with neoclassical predicitons.

To summarize, one of the principal findings of the Magnetic Fusion Advisory Committee is that there continues to be notably broad and vigorous technical progress in the consolidation of the scientific and technological data base for magnetic fusion. The U.S. magnetic fusion program has reached its present state of world leadership by capitalizing on a series of scientific successes, with new facilities following up promising technical developments, while discarding less promising concepts. This policy has produced a balanced program directed at: (a) the world's first D-T demonstration of scientific feasibility, expected in the Tokamak Fusion Test Reactor, (b) facilities, including the Mirror Fusion Test Facility, partly constructed, that will allow a comparative evaluation of the two main variants of magnetic confinement geometries (closed and open systems), (c) continued exploration of alternative concepts, and (d) test facilities exploring critical technology issues such as superconducting magnets and the handling of tritium. It is this broad-based but tightly integrated research program that provided the basis for an aggressive pursuit of the engineering development of fusion envisioned in the Magnetic Fusion Engineering Act of 1980. However, while foreign programs, especially in Japan, continue at a vigorous pace, budget pressures have begun to erode the U.S. program, causing stretchout of research on the mainline approaches, lengthy delays in facilities for alternative concepts and for advanced technologies, and elimination of engineering development activities envisioned at the Center for Fusion Engineering. The Magnetic Fusion Advisory Committee recommends that DOE take immediate steps in the FY84 budget process to restore program momentum and to return to an accelerated course of development consistent with the significant technical progress outlined above. This should be at least the "program-driven" or "enhanced" level outline in Sec. 2 and Appendices B and C.

B. Principal Findings and Recommendations

The Panel reports and recommendations (Appendices B and C) and extensive discussions and presentations at the August 30 and 31 MFAC meeting, form the basis for the following principal findings and recommendations by the Magnetic Fusion Advisory Committee:

B.1 Tandem Mirrors and Tokamaks

- At present, the tandem mirror and tokamak concepts can be embodied in viable reactor designs of roughly similar characteristics. In both reactor designs, there are scientific and technological assumptions that remain to be validated by experiment. Both designs also have the potential for significant further improvement through technical innovations that are in the exploratory phase.

- In the case of the tokamak, the existing data base implies the feasibility of net power production. Demonstration of the achievability of sufficiently high power density and long pulse length for commercial attractiveness is being addressed by the experimental program. Steady-state current drive at low recirculating power is a promising option for concept improvement.

- Tandem mirror research is at an earlier phase of development. The current experimental program is addressing the feasibility of net power production. In this context, the demonstration of the thermal barrier technique on TMX-Upgrade is of particular importance. The tandem mirror concept offers the possibility of driven steady-state, high-beta operation.

- The TFTR facility is expected to demonstrate the scientific feasibility of magnetic fusion during the mid 1980s. In order to continue high-performance tokamak experimental progress during the first part of the 1990s, an appropriate upgrade of the TFTR facility should be considered for operation in this period. This topic is being addressed by MFAC Panel #3 evaluating upgrade options for TFTR.

- The development of the required data base for the tandem mirror reactor concept should be pursued vigorously. Essential to this is the timely completion of the MFTF-B facility now under construction. Future upgrade options for MFTF-B are now under study, and will be evaluated by MFAC Panel #3.

- The development of tandem mirrors and tokamaks has greatly benefited from innovative research on medium- and small-scale experimental facilities, and vigorous support activities in development and technology, plasma theory, and computations. Continued high priority should be given to these activities.

- The relative promise of the tandem mirror and tokamak reactor concepts should be reassessed periodically on the basis of new experimental data. A preliminary assessment can be made at the end of 1984. A more substantive assessment can be made at the end of 1987 in the "program-driven" or "enhanced" (nominal $600M) case.† The program funding balance should be readjusted as may be appropriate on the basis of these technical assessments.

- If technical developments continue to be favorable, the decision to proceed with the construction of an Engineering Test Reactor (ETR) of either the tokamak or tandem mirror type could be made at the end of 1987 in the "enhanced" (nominal $600M) case, with D-T operation scheduled for the latter part of the 1990s. Competing conceptual design studies should be undertaken in the near future, with intensified efforts as appropriate following the 1984 assessment.

- Depending on the pace and timing of ETR, qualitative comparisons can be made with the FED and INTOR design objectives. At the present time, however, MFAC notes that only a rudimentary scoping exists of ETR goals and performance characteristics (Appendix B). This underscores the importance of undertaking ETR conceptual design studies as soon as possible.

- The program plans†† (Appendix B) in the "enhanced" (nominal $600M) case have high technical merit both with regard to program content and prospects for establishing the tandem mirror and tokamak data base required for initiation of ETR construction at the end of 1987.

- In the "constrained" (nominal $500) case, the program plans (Appendix B) maintain an internal logic similar to the "enhanced" (nominal $600M) case, with focus on critical physics and technology issues but a concomitant stretch-out by several years of the preparation phase for ETR. Actual implementation of the ETR would require incremental funding. The "constrained" case is marked by a significant increase in technical risk in establishing the necessary data base for an early ETR decision.

- In the "reduced" (nominal $400M) case, the program plans have not been formulated and analyzed in sufficient detail to permit specific recommendations by MFAC at this time. However, it is evident that the "reduced" budget case would have a major negative impact on overall program pace and U.S. leadership in fusion energy development, and would require a dismantling of key elements of the present goal-oriented program.

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† Here, the "enhanced" program refers to the nominal "$600M" case discussed by the panels. In this regard, it should be noted that the "enhanced" budget total in the "out" years (beyond FY84) exceeds $600M, depending on the pace of major facilities.

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†† Throughout Sec.2.B, the term "program plans" refers to the program plans developed and summarized by Panel #1 for tandem mirrors and tokamaks (Appendix B) and by Panel #2 for Elmo bumpy tori, reversed field pinches and stellarators (Appendix C).

B.2 Elmo Bumpy Tori, Reversed Field Pinches and Stellarators

- Research on the backup concepts has resulted in significant advances in the basic physics understanding of plasma confinement properties. Moreover, the technology development and innovative reactor features associated with the backup concepts have had a favorable impact on the development of the mainline approaches. Even at the "reduced" (nominal \$400M) budget level, it is important that at least the present fraction (approximately 15%) of the confinement effort be devoted to experimentation on backup concepts not motivated by mainline ETR considerations. The physics and technology payoff of such independent programs is large.

- Each of these confinement approaches, while at an earlier stage of scientific development, is characterized by a rapidly evolving and encouraging data base (Sec. 2.A and Appendix C). Each concept, to varying degrees, also offers potential reactor advantages (e.g., high power density, steady-state operation, high modularity, etc., depending on the concept) relative to the mainline approaches.

- The Elmo bumpy torus is based on physics principles relating to transport that are different from those in the mainline. It offers a product qualitatively similar to those in the mainline, namely, a low-to-moderate power density, large-scale power reactor, but with the potential advantages of steady-state and high modularity. The realization of this potential requires large extrapolations of the classical-like behavior observed in the present EBT-S experiments.

- The reversed field pinch has the potential to produce a qualitatively different reactor product for the fusion program, namely a high-power-density, high-beta, compact reactor with prospects for ignition solely with ohmic heating. Demonstration of sustained, stable operation at high beta and favorable confinement scaling at high current are essential requirements for achieving this reactor potential. Large extrapolations of confinement behavior are required in considering reactor potential.

- The conventional stellarator has the most advanced scientific data base of all backup concepts. Most stellarator reactor designs lead to a product qualitatively similar to those of the mainline approaches (namely, low-to-moderate power density and high output power), with the potential advantages of low recirculating power and steady-state, disruption-free operation. At the present time, stellarator reactor coil systems appear to be technically more complex than those of other toroidal concepts. Recent advanced stellarator concepts (such as helical axis configurations) offer the prospect of high beta and high power density.

- The proof-of-principle (POP) program plans (Appendix C) are based on an examination of the present scientific and technological basis for each concept. Except in the case of highly accelerated development, no one of the backup concepts can be technically ready for an ETR decision point as early as the end of 1987. However, any of the backup concepts that continues to show strong technical merit through the POP phase could be further developed in post-POP facilities for an FDP* decision point in the late 1990s.

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* Fusion Demonstration Plant

- Program emphasis should be placed on the development of backup concepts that show significant promise of an improved reactor product as well as a strong scientific data base.

- In the "enhanced" (nominal $600M) case, the program plans for all three backup concepts (Appendix C) have strong technical merit. However, it appears that resources would realistically permit (a) POP development of at most two concepts in the "enhanced" (nominal $600M) case, assuming that the mainline program remains at the "constrained" (nominal $500M) level, or (b) POP development at a reduced pace of at most one concept in the case in which the overall fusion program is "constrained" (nominal $500M).

- In the "reduced" (nominal $400M) case, MFAC concludes that reduced budgets do not permit advancement of any of the backup concepts to the POP level of development. It is clear that the "reduced" budget case would have a large negative impact on development of the backup concepts. Nonetheless, exploratory research programs should be maintained on existing facilities (e.g., EBT-S and ZT-40) with particular emphasis on innovative concepts and improved understanding of confinement properties. Stellarator research should continue at the "study" level, with the possibility of undertaking modest experiments.

- Assuming continued strong technical progress, further development of the backup concepts is recommended in the "enhanced" (nominal $600M) and "constrained" (nominal $500M) cases. The pace of development should be based on technical merit and availability of funds.

- Based on continued technical progress in ETB-S and the findings of the EBT checkpoint review panel (July, 1981), the Elmo bumpy torus is technically ready to proceed to the next development phase (Appendix C). As stated earlier, it offers a product qualitatively similar to those in the mainline, namely, a low-to-moderate power density, large-scale power reactor, but with the potential advantages of steady-state operation and high modularity. In this regard, it offers an improved reactor product. At the "constrained" (nominal $500M) level, MFAC recommends that the EBT-P project be reviewed with the intent of finding an approach which substantially reduces annual funding requirements while still addressing critical physics issues, probably at less than POP conditions. The Committee recommends that if such an approach can be identified, high priority should be given to its pursuit without additional delay, assuming favorable technical reviews. At the "enhanced" (nominal $600M) level it is recommended that strong consideration be given to going ahead with the EBT-P project as currently planned.

- The recent technical progress in the reversed field pinch data base has been substantial (Sec. 2.A and Appendix C). Moreover, as stated earlier, the reversed field pinch has the potential to produce a qualitatively different reactor product, namely a high-power-density, high-beta, compact reactor with prospects for ignition solely with ohmic heating. For these reasons, high priority should be given to further development of the reversed field pinch concept. Favorable confinement scaling with increased current is important and should be evaluated in ZT-40 as soon as possible.

In parallel, there should be an assessment of the engineering issues related to the compact high-power-density aspects of reversed field pinch experiments and reactor designs. The reversed field pinch "reference" program (Appendix C) has technical merit for the "constrained" (nominal $500M) case while the reversed field pinch "enhanced" program (Appendix C) appears appropriate to the "enhanced" (nominal $600M) case.

- Based on present understanding, the conventional stellarator leads to a reactor product qualitatively similar to those of the mainline approaches, with exceptionally low requirements for recirculating power but with difficult engineering features related to coil design (Appendix C). Such conventional stellarator approaches are being actively pursued in Japan, the Federal Republic of Germany and the U.S.S.R.. The Magnetic Fusion Advisory Committee recommends that theoretical studies of the novel stellarator designs which offer higher beta be pursued and evaluated as advanced concepts if such designs emerge. Because of the relatively advanced stellarator data base, and the prospects for current-free, steady-state operation, stellarator upgrade options are under active consideration as toroidal "concept improvements" in some of the existing tokamak facilities (e.g., ISX-B and PDX). In any such conversion, priority should be given to incorporating advanced features that offer the prospects of attractive plasma beta and reactor embodiment, and to advancing the basic understanding of toroidal confinement.

B.3 International Cooperation

The fusion program in the United States has significantly benefited from research in other nations. Moreover, fusion research and development has tradionally been characterized by active international cooperation and exchange of ideas and information that has led to rapid technical progress. The most striking example of this is the rapid U.S. and world-wide development of the tokamak concept originally investigated in the Soviet Union. Of course, other nations' fusion programs also benefit from research activities and concepts developed in the U.S.. For example, the Stellarator approach, originally developed in the U.S., is now actively pursued in Europe, Japan and the Soviet Union. International cooperation in fusion takes place not only at the level of exchange of ideas and information. The joint U.S.-Japan research program on the Doublet III tokamak is an excellent example of a productive international collaboration on a major research facility. The INTOR studies provide an excellent example of multinational advanced design activity. On a smaller scale, the multinational collaboration on the TEXTOR tokamak in the Federal Republic of Germany has also been productive.

For each of the confinement approaches considered here, the Panel reports (Appendices B and C) give an excellent delineation of related programs and facilities in other nations. A characterization of the present international level of effort on each confinement approach is summarized in the table below.

	U.S. Effort	Effort in Other Nations
Tokamak	Major	Major (Europe, Japan, Soviet Union)
Tandem Mirror	Major	Moderate (Japan, Soviet Union)
Elmo Bumpy Torus	Moderate	Modest (Japan)
Reversed Field Pinch	Moderate	Moderate (Europe, Japan)
Stellarator	Very Modest (mostly study activities)	Moderate (Europe, Japan, Soviet Union)

For each concept, the U.S. effort is largely complementary to activities in other nations.

The Magnetic Fusion Advisory Committee offers the following brief summary of principal findings and recommendations with regard to international cooperation in fusion.

- The U.S. fusion program and the development of fusion on a world-wide basis has significantly benefited from the active exchange of information and ideas. International cooperation in fusion research should continue to receive strong emphasis in the U.S. program.

- To a large extent, the design of fusion facilities and the evolution of program plans on a world-wide basis have placed emphasis on investigations of complementary technical issues. This minimization of duplication is both cost-effective and conducive to rapid technical development. It encourages broader coverage of options in the area of alternate concepts and allows larger steps to be taken in the mainline approaches within existing budgetary constraints.

- While maximum effective use should be made of research facilities abroad, the overall priorities of the U.S. program should continue to be guided by the technical outlook.

- Worldwide breadth of effort will require increased international consultation in program planning.

Department of Energy
Washington, D.C. 20585

June 1, 1982

Dr. Ronald C. Davidson
Director, Plasma Fusion Center
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, MA 02139

Dear Ron:

The Department has several issues that must be addressed in the process of preparing its budget recommendation regarding magnetic fusion research and development. To assist the Department in this process I would like the Magnetic Fusion Advisory Committee (MFAC) to address some of these and provide recommendations where appropriate. In order to be most useful and to match our planning cycle, I would like a preliminary report within the next three months and a final report within six months. For the various issues, the committee should consider three hypothetical budgets, ($400M-$500M-$600M), which for the foreseeable future should be regarded as constant effort levels. In qualitative terms, these could be regarded as declining, constant, and growing programs.

1. It now appears that only steady-state tokamaks and tandem mirror reactor concepts will be at similar stages of development with respect to their potential as reactor candidates by 1986-87. Please review and comment on the makeup and pace of the tandem mirror and the steady-state tokamak programs as they contribute to the scientific and technological base needed to evaluate their possible consideration as candidates for an Experimental Test Reactor (ETR) Project within the next decade. Also, please assess the completeness of the experimental facilities now planned to bring these concepts to such a comparison stage, and make recommendations as to the most complete program allowed by the above budget levels. In addition to establishing the scientific base for an ETR, what priority should be given to technology development needed to establish a technical base for building an ETR prior to the ETR decision?

2. In addition to these two mainline reactor programs, some backup program should be continued on a longer time scale. Please review the Stellarator, Elmo Bumpy Torus, and Reversed Field Pinch concepts to advise us of your views on their relative priority as backups to the mainline steady-state tokamak and tandem mirror reactor development programs. In your deliberations and investigations of these concepts, please take the following into account:

- o Reactor characteristics
- o Scientific base for a Proof of Principle (POP) Experiment
- o Technology base for a POP Device
- o Development needs of a POP Device and the size and timing of a program to complete this development

- o Evaluation of international programs to provide similar information to that generated by a U.S. development program of the backup concept
- o Nature and size of post-POP facilities needed to prepare a technical base for an ETR application

Make a recommendation as to the desirability and choices of backup concepts in the above budget cases.

3. Various program reviews have recommended an increased emphasis on engineering technology. Although there are various ways to develop component technologies for fusion reactors, experience in other development programs shows the value of utilizing a complete system as a technology test facility. The Fusion Engineering Device (FED) would have provided a system test. The high cost of the FED precludes construction at this time. We are attempting to find other means to approximate this systems integration function of the FED. It has been proposed that the TFTR facility be utilized for this purpose, even though there are other possible uses for this facility.

I would like the MFAC to consider the possible programmatic roles of the TFTR facility after feasibility experiments and to make recommendations with respect to its use as: (1) an engineering development facility (TFET); (2) an alpha particle physics experiment (TFM); (3) a reactor-level hydrogen experiment; or (4) other.

These are difficult and important issues to which I am sure your committee will devote a great deal of effort. Although I have outlined a collection of specific items for which we have the clear need for near-term advice, the committee is encouraged to identify issues that it believes should also be addressed. They could be in the form of recommendations from which we could develop specific items for committee consideration.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

cc: Deputy Secretary
Under Secretary
John Clarke, ER-50

MEMORANDUM TO: John Gilleland

FROM: Ron Davidson

SUBJECT: Charge to Review Panel on Tandem Mirrors and Tokamaks

DATE: June 7, 1982

Thank you for agreeing to chair the review panel on Tandem Mirrors and Tokamaks.

Enclosed is a copy of the letter from Al Trivelpiece outlining three areas for evaluation and recommendation by the Magnetic Fusion Advisory Committee (MFAC). The charge to your panel relates to formulating recommendations to MFAC on Item #1:

> "It now appears that only steady-state tokamaks and tandem mirror reactor concepts will be at similar stages of development with respect to their potential as reactor candidates by 1986-87. Please review and comment on the makeup and pace of the tandem mirror and the steady-state tokamak programs as they contribute to the scientific and technological base needed to evaluate their possible consideration as candidates for an Experimental Test Reactor (ETR) Project within the next decade. Also, please assess the completeness of the experimental facilities now planned to bring these concepts to such a comparison stage, and make recommendations as to the most complete program allowed by the above budget levels. In addition to establishing the scientific base for an ETR, what priority should be given to technology development needed to establish a technical base for building an ETR prior to the ETR decision?"

As discussed at the June 2 meeting of MFAC, preparation of the panel report and recommendations relating to this charge should be carried out within the context of the following general guidelines.

1. Review and make recommendations regarding the scientific and technological basis and program plans for each concept. Identify the strengths and weaknesses of each confinement approach as a practical fusion reactor.

2. Delineate the nature and role of large-, medium- and small-scale facilites leading to an ETR decision point, and identify the supporting technology and applied physics activities necessary to implement program plans.

3. Address the role of international cooperation and technical information from fusion facilities in other nations in determining emphasis and priorities in the U. S. program.

4. Develop program plans and recommendations for two budget cases:

(a) Flat budget case (in constant dollars), corresponding to $400 M, $500 M or $600 M the for overall magnetic fusion program in FY84 and beyond.

(b) Enhanced budget case (but limited to an average real growth of 10% per year, say), corresponding to an accelerated ETR/FED decision point consistent with what is technically feasible.

To the maximum extent possible, realistic cost estimates are to be used in formulating program plans.

5. For those concepts that cannot compete for a particular ETR decision point, plans should be directed at concept optimization for the Fusion Demonstration Plant (FDP).

With regard to guideline #4, based on input from the three review panels, it should be kept in mind that MFAC will ultimately formulate recommendations to DOE within a total budget envelope for the overall magnetic fusion program.

As a general remark, given the implications and magnitude of the issues, it is very important that panel deliberations be thorough and that recommendations be technically and programatically sound.

Good luck in this important undertaking. If there is any way I can be of assistance, don't hesitate to contact me.

RCD:mg

xc: Review Panel on Tandem Mirrors and Tokamaks
Magnetic Fusion Advisory Committee
MFAC Executive Secretary

REVIEW PANEL ON TANDEM MIRRORS AND TOKAMAKS

Dr. John Gilleland, Chairman
General Atomic Company

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Argonne National Laboratory

Dr. David Baldwin
Lawrence Livermore National Laboratory

Prof. James Callen
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Prof. John Dawson
University of California at Los Angeles

Dr. Harry Dreicer
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Dr. Peter Staudhammer
TRW

[†] Chairman, subpanel on tandem mirrors
[††] Chairman, subpanel on tokamaks

Plasma Fusion Center
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

MEMORANDUM TO: Bob Conn

FROM: Ron Davidson

SUBJECT: Charge to Review Panel on the Elmo Bumpy Torus, Reversed Field Pinch and Stellarator Concepts

DATE: June 7, 1982

Thank you for agreeing to chair the review panel on the Elmo Bumpy Torus, Reversed Field Pinch and Stellarator Concepts.

Enclosed is a copy of the letter from Al Trivelpiece outlining three areas for evaluation and recommendation by the Magnetic Fusion Advisory Committee (MFAC). The charge to your panel relates to formulating recommendations to MFAC on Item #2:

> "In addition to these two mainline reactor programs, some backup program should be continued on a longer time scale. Please review the Stellarator, Elmo Bumpy Torus, and Reversed Field Pinch concepts to advise us of your views on their relative priority as backups to the mainline steady-state tokamak and tandem mirror reactor development programs. In your deliberations and investigations of these concepts, please take the following into account:
>
> - Reactor characteristics
> - Scientific base for a Proof of Principal (POP) Experiment
> - Technology base for a POP Device
> - Development needs of a POP Device and the size and timing of a program to complete this development
> - Evaluation of international programs to provide similar information to that generated by a U.S. development program of the backup concept
> - Nature and size of post-POP facilities needed to prepare a technical base for an ETR application
>
> Make a recommendation as to the desirability and choices of backup concepts in the above budget cases."

As discussed at the June 2 meeting of MFAC, preparation of the panel report and recommendations relating to this charge should be carried out within the context of the following general guidelines.

1. Review and make recommendations regarding the scientific and technological basis and program plans for each concept. Identify the strengths, weaknesses and primary characteristics of each confinement approach as a practical fusion reactor.

2. Delineate the nature and role of large-, medium- and small-scale facilites leading to an ETR decision point, and identify the supporting technology and applied physics activities necessary to implement program plans.

3. Address the role of international cooperation and technical information from fusion facilities in other nations in determining emphasis and priorities in the U. S. program.

4. Develop program plans and recommendations for two budget cases:

   (a) Flat budget case (in constant dollars), corresponding to $400 M, $500 M or $600 M for the overall magnetic fusion program in FY84 and beyond.

   (b) Enhanced budget case (but limited to an average real growth of 10% per year, say), corresponding to an accelerated ETR/FED decision point consistent with what is technically feasible.

   To the maximum extent possible, realistic cost estimates are to be used in formulating program plans.

5. For those concepts that cannot compete for a particular ETR decision point, plans should be directed at concept optimization for the Fusion Demonstration Plant (FDP).

With regard to guideline #4, based on input from the three review panels, it should be kept in mind that MFAC will ultimately formulate recommendations to DOE within a total budget envelope for the overall magnetic fusion program.

As a general remark, given the implications and magnitude of the issues, it is very important that panel deliberations be thorough and that recommendations be technically and programatically sound.

Good luck in this important undertaking. If there is any way I can be of assistance, don't hesitate to contact me.

RCD:mg
xc: Review Panel on Elmo Bumpy Torus, Reversed Field Pinch
    and Stellarator Concepts
  Magnetic Fusion Advisory Committee
  MFAC Executive Secretary

REVIEW PANEL ON ELMO BUMPY TORUS, REVERSED FIELD PINCH AND STELLARATOR CONCEPTS

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University of California at Los Angeles

Prof. Robert Gross, Vice Chairman
Columbia University

Dr. Fred Coensgen
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Dr. Nicholas Krall
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Mr. Donald Kummer
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Dr. Rulon Linford
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Dr. Robert Miller
Los Alamos National Laboratory

Prof. Leon Shohet
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Dr. Igor Sviatoslavsky
University of Wisconsin

Prof. Harold Weitzner
New York University

Dr. Shoichi Yoshikawa
Princeton University

MAGNETIC FUSION ADVISORY COMMITTEE

SUMMARY OF FINDINGS AND RECOMMENDATIONS ON TFTR UPGRADE OPTIONS

Submitted to

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C., 20585

January 11, 1983

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J.R. Gilleland, GA

R.A. Gross, Columbia U.

D.L. Kummer, MDAC

R.K. Linford, LANL

P. Murray, Westinghouse

M.N. Rosenbluth, U. Texas

M.W. Rosenthal, ORNL

M. Tigner, Cornell U.

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January 11, 1983

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory met in Livermore on January 10 and 11, 1983, and reviewed the findings and recommendations of the MFAC Panel evaluating TFTR upgrade options (Panel #3). The final Panel report (enclosed) and summary of Panel findings and recommendations (attached) were accepted in principle by MFAC and are transmitted to you at this time with the following ancillary comments and recommendations:

1. The Magnetic Fusion Advisory Committee agrees that the near-term demonstration of ignition and long-pulse equilibrium burn constitutes the technically necessary next major step for tokamaks beyond the TFTR break-even demonstration.

2. The Committee notes that such a step represents an evolution in the strategy of the tokamak program plan defined by Panel #1. The mission of the TFTR Upgrade is raised to the level of a Tokamak Fusion Core Demonstration (TFCD). The scope of the tokamak Engineering Test Reactor (ETR), as the last step before commercialization, can be enhanced correspondingly.

3. In order to provide a reactor-relevant configuration on the road to a tokamak ETR, a superconducting-coil device such as DCT-8 is highly desirable. However, lower-cost options for the achievement of ignition and long-pulse burn, using copper coils, should continue to be investigated. The technical merit of copper-coil options relative to superconducting-coil options should be determined in the light of budgetary constraints and technological needs.

4. While the construction costs for an equilibrium burn experiment of the DCT-8 type are substantially smaller than those for the FED or for a tokamak ETR, it is clear that incremental funding above the base program level will be required for timely implementation.

5. Following a review of MFTF-B upgrade options, it is recommended that MFAC integrate and prioritize the findings and recommendations regarding tokamaks and tandem mirrors (Charge #1), alternate fusion concepts (Charge #2), TFTR upgrade options (Charge #3) and MFTF-B upgrade options (Charge #4) within the context of the overall fusion program strategy and projected fusion budgets.

You may also wish to review the minutes of the January 10th and 11th MFAC meeting for useful supplementary discussions and information regarding possible TFTR upgrade options.

If there are any questions, do not hesitate to contact me.

Sincerely,

Ron Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee

RCD:dlr

xc: Magnetic Fusion Advisory Committee
MFAC Executive Secretary

SUMMARY OF
FINDINGS AND RECOMMENDATIONS OF THE MFAC PANEL EVALUATING
TFTR UPGRADE OPTIONS

FINDINGS

1. The TFTR facility constitutes a unique resource in the U.S. fusion program, and full advantage should be taken of this asset. Cost-effective improvements to TFTR, both short-run and long-run, are to be encouraged.

2. The data base in the tokamak program is sufficiently promising that meaningful steps beyond TFTR should receive serious consideration. However, without a significant physics and technology step in the tokamak program beyond TFTR, commitment to the ETR which would achieve net power generation at reasonable availability can not be technically justified.

3. The next important steps in the tokamak program are to demonstrate:

   - Plasma ignition and equilibrium burn control.
   - Reactor-relevant engineering configuration.
   - Long-pulse or quasi-steady-state operation.

   Accomplishing all of these steps in a single, near-term device would require incremental funding beginning in FY 85 and beyond to at least the enhanced ($600 M) budget case.

4. The TFTR site is compatible with a variety of significant improvements and upgrades, including ignition and pulsed equilibrium burn D-T options as well as long-pulse and quasi-steady-state hydrogen plasma options. The TFTR vacuum chamber and toroidal field coils must be replaced in all of the high-Q options.

5. Among the options considered, the most attractive from a technical standpoint is the PPPL proposed option (termed here "DCT-8")

consisting of a superconducting D-T burning device capable of ignition and very long-pulse operation. Apparent cost and performance advantages are achieved in comparision with the FED reactor design by the use of a hybrid toroidal coil system, with copper insert coils providing an increment above the fields produced by the superconducting coils.

6. The DCT-8 option would permit a thorough characterization of the plasma core of a fusion reactor and would incorporate many reactor prototypical engineering features. Moreover, it would provide renewed impetus to fusion research and would facilitate a more aggressive community stance with respect to ETR objectives.

7. Incorporation of a high neutron fluence capability into any TFTR upgrade option appears to be precluded by both cost and site limitations. Consequently, developing the nuclear engineering data base to support a commitment to a fusion reactor will require additional facilities.

RECOMMENDATIONS

1. The technically preferred option, a superconducting long-pulse ignition device, such as DCT-8, should be studied in detail by a group with broad community representation to permit informed design decisions and to develop definitive estimates of cost, schedule, and supporting R&D requirements.

2. The budgetary and programmatic implications of a near-term commitment to a DCT-8 device should be explored in depth. Further work is also needed to specify the desired prerequisites and complements to this facility and to investigate the issues related to cost, schedule, industrial participation, and facility management.

3. The advantages and disadvantages of siting the facility at PPPL should be quantified. While considerable benefit would be realized if the siting were at Princeton (e.g., the TFTR staff, hardware, and

infrastructure), this advantage must be weighed against the benefits derived by adopting a new location that would be suitable for an ETR as well. In addition, particular attention should be focused on the limitations of the nuclear mission as determined from TFTR site constraints.

4. Less ambitious TFTR improvement and upgrade options, specifically those that focus primarily on either the physics or the technology issues, should continue to be studied in order that meaningful comparisons may be made and to provide a backup in the event that budgetary stringency precludes proceeding with the technically preferred option. Both long-pulse superconducting hydrogen options that incorporate reactor-relevant engineering features and copper D-T options that can achieve long-pulse ignited operation are regarded as highly valuable program elements. The panel was divided on the question of which of these two classes of options is the more meritorious.

5. Since the nuclear engineering requirements of fusion reactors cannot be met solely by the proposed upgrades of the TFTR, a serious parallel effort involving complementary facilities must be undertaken to develop the nuclear data base.

6. A plan that takes into consideration the overall physics, technology, and engineering needs of the tokamak program must be drawn up. These needs, when considered in conjunction with the existing facilities, experiments, and personnel resources, should be used to identify and investigate any outstanding critical technical issues prior to any TFTR upgrade and certainly before any ETR design. Planning to the level of detail envisioned here should be given a higher priority.

7. Industrial involvement and integration into the program should be undertaken wherever and whenever possible if the industries are to be prepared to make meaningful contributions in the ETR era and beyond. Such participation in all TFTR upgrade options should be factored into the program plan.

REVIEW PANEL EVALUATING TFTR UPGRADE OPTIONS

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Bechtel

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Argonne National Laboratory

Dr. Raymond F. Beuligmann
General Dynamic

Dr. Daniel R. Cohn
Massachusetts Institute of Technology

Dr. John E. Cummings
EPRI

Prof. Gerald L. Kulcinski
University of Wisconsin

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Dr. O.B. Morgan
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Prof. Maury Tigner
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MEMORANDUM TO: Harold Forsen

FROM: Ron Davidson

SUBJECT: Charge to Review Panel on Programmatic Roles of Upgraded TFTR Facility

DATE: June 7, 1982

Thank you for agreeing to chair the review panel on possible programmatic roles of an upgraded TFTR facility.

Enclosed is a copy of the letter from Al Trivelpiece outlining three areas for evaluation and recommendation by the Magnetic Fusion Advisory Committee (MFAC). The charge to your panel relates to formulating recommendations to MFAC on Item #3:

> "Various program reviews have recommended an increased emphasis on engineering technology. Although there are various ways to develop component technologies for fusion reactors, experience in other development programs shows the value of utilizing a complete system as a technology test facility. The Fusion Engineering Device (FED) would have provided a system test. The high cost of the FED precludes construction at this time. We are attempting to find other means to approximate this systems integration function of the FED. It has been proposed that the TFTR facility be utilized for this purpose, even though there are other possible uses for this facility.
>
> I would like the MFAC to consider the possible programmatic roles of the TFTR facility after feasibility experiments and to make recommendations with respect to its use as: (1) an engineering development facility (TFET); (2) an alpha particle physics experiment (TFM); (3) a reactor-level hydrogen experiment; or (4) other."

As discussed at the June 2 meeting of MFAC, preparation of the panel report and recommendations relating to this charge should be carried out within the context of the following general guidelines.

1. The "other" category in the Item (4) of the above charge can cover options ranging from no upgrade of TFTR to an upgraded ignition test facility (for example).

2. Panel should delineate possible roles of Industry in fabrication and operation of TFTR upgrade options.

3. Recommendations and program plans for upgrade options should be developed for two budget cases:

(a) Flat budget case (in constant dollars), corresponding to $400 M, $500 M, or $600 M for the overall magnetic fusion program in FY84 and beyond.

(b) Enhanced budget case (but limited to an average real growth of 10% per year, say), corresponding to an accelerated ETR/FED decision point consistent with what is technically feasible.

To the maximum extent possible, realistic cost estimates are to be used in formulating plans and recommendations.

Your panel may also wish to address the role of international cooperation and technical information from fusion facilities in other nations in preparing your report and recommendations.

With regard to guideline #3, based on input from the three review panels, it should be kept in mind that MFAC will ultimately formulate recommendations to DOE within a total budget envelope for the overall magnetic fusion program.

It is my understanding that the DOE technical review of TFET will be held at a later date than originally planned. You may therefore wish to encourage panel members to attend the Princeton information meeting on this subject scheduled for June 24 and 25.

As a general remark, given the implications and magnitude of the issues, it is very important that panel deliberations be thorough and that recommendations be technically and programmatically sound.

Good luck in this important undertaking. If there is any way I can be of assistance, don't hesitate to contact me.

RCD:mg

xc: Review Panel on Programmatic Roles of Upgraded TFTR Facility
Magnetic Fusion Advisory Committee
MFAC Executive Secretary

MAGNETIC FUSION ADVISORY COMMITTEE

SUMMARY OF NEW CHARGE AREAS

January 11, 1983

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Department of Energy
Washington, D.C. 20585

January 6, 1983

Dr. Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee
Director, Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street, NW16-202
Cambridge, MA 02139

Dear Ron,

I have read the Report on Evaluation of Magnetic Confinement Concepts submitted to the Department on November 1, 1982, by the Magnetic Fusion Advisory Committee (MFAC). The Report assisted us in developing budgets for the coming year. Thank you for your efforts in producing a fine piece of work.

Now, I would like your advice in two additional areas. The first is on the role of MFTF-B, and the second is on the role of universities in the fusion program.

In my June 1, 1982 letter to the Committee, I requested MFAC to consider programmatic roles for the TFTR, following feasibility experiments. It is also important that the other major facility, MFTF-B, be utilized to the greatest benefit of the fusion program. Therefore, as an extention of my charge #3 to MFAC last June, I would like the Committee to consider possible programmatic roles for MFTF-B following the completion of its initial experimental program.

In your deliberations, you should take into consideration the possibility of technology-oriented roles for MFTF-B, as well as physics-oriented roles. Facilities upgrade modifications should be included in your deliberations, in analogy with the charge on TFTR.

I would like your final report on this topic by April 1983.

To assist the Department in making effective use of its university-based fusion research program, I would like MFAC to advise us on the long-term role of universities in the fusion program.

Fusion research involves the triad of universities, national laboratories, and industry. Because of the evolutionary nature of the fusion program, the role of each of them will likely change. Therefore, emphasis should be put on exploring new forms of participation, rather than solely changing the level. The Office of Fusion Energy has the responsibility to ensure that these changes strengthen the overall program.

At present, universities are generally involved with smaller scale exploratory research. However, the approach toward reactor conditions tends to imply larger and more costly physics and engineering experiments, as well as more focused and detailed reactor studies. Therefore, these differences must be taken into account as universities review their programs.

Considering the evolving nature of the fusion program, what could the Office of Fusion Energy do, and what could universities do to maintain strong and productive university-based research?

Furthermore, the magnetic fusion energy program, will need a continuing supply of creative, appropriately trained scientists and engineers. A recent manpower study performed for the Office of Fusion Energy indicates that sufficient numbers of technical personnel are likely to be available in the coming decades, for certain specific technical disciplines. However, fusion requires the application of general technical expertise to specific fusion problems as well as certain specialized skills in these areas. What actions could the Office of Fusion Energy take now to be certain that universities will, in fact, be able to produce the types and quality of personnel needed by the fusion program in the future?

The Energy Research Advisory Board (ERAB) has been asked to examine the broad issue of universities' research and development activities, and to address specifically, the effectiveness of the Department's policies and working relationships with the academic community. A copy of the Deputy Secretary's charge letter to ERAB is enclosed.

I would like your final report on this topic by July 1983.

I consider these to be very significant issues for the Department and I look forward to receiving the advice of your Committee.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

Enclosure

Department of Energy
Washington, D.C. 20585

November 5, 1982

Mr. Louis H. Roddis, Jr.
Chairman, Energy Research
Advisory Board
110 Broad Street
Charleston, SC 29401

Dear Lou:

The Energy Research Advisory Board recently completed an excellent study of the Department's multiprogram laboratories. Your recommendations on ways to improve the effectiveness of these important institutions are being carefully and thoroughly considered.

National progress in fundamental research and other areas of science and engineering depends very substantially on the effectiveness of the Department's use of all available scientific and technical resources; laboratory, university and industry personnel. The Department currently provides about $300 million in direct support of university research, and about an equal amount in indirect support for activities at universities that contribute to our missions. It is essential to ensure that university research and manpower development activities are adequate, effective and consistent with the Department's missions and the needs of our energy R&D programs.

I request, therefore, that ERAB examine the broad issue of the roles and missions of universities' research and development and specifically address the effectiveness of the Department's policies toward and working relationships with the academic community. Within this framework, the Board should explore, among others, the following questions:

- Is the Department effectively utilizing the general capabilities of the university community in both fundamental and energy research?

- Is the Department effectively utilizing universities, vis-a-vis laboratory and private sector groups, in energy research and technology development programs?

- What responsibilities and mechanisms are appropriate for the Department in professional level manpower development including, but not limited to, support for graduate students, postdoctoral researchers, and younger faculty members?

- What current factors, actual or perceived, foster or impede good working relationships between the Department and the academic community.

- In what ways, if any, could the Department improve the effectiveness of the working relationships between the DOE laboratories and the universities including, for example, the planning and conduct of joint research programs?
- What changes, if any, should the Department make in order to provide the most effective support of university-based energy research?

These are only a sample of the obviously complex issues the Board may wish to address. You can be assured of the full cooperation and assistance of the Department as you plan and carry out this study. The schedule will, of course, be established by the Board in relation to other priorities. However, I would appreciate a final report by August 1983.

By copy of this letter, I am requesting the Director of the Office of Energy Research, the Assistant Secretaries and the Operations Office Managers to give you their fullest cooperation and support. Staff assistance for this study will be provided by the Office of Energy Research.

I look forward to discussing this subject with you and receiving your report and recommendation.

Sincerely,

W. Kenneth Davis
Deputy Secretary

MEMORANDUM TO: Bob Gross

FROM: Ron Davidson RCD

SUBJECT: Charge to Review Panel Assessing the Long-Term Role of Universities in the Fusion Program

DATE: January 11, 1983

Thank you for agreeing to chair the review panel assessing the long-term role of Universities in the fusion program.

Enclosed is a copy of the January 10, 1983 letter from Dr. Alvin W. Trivelpiece outlining two areas for evaluation and recommendation by the Magnetic Fusion Advisory Committee (MFAC). The charge to your panel is to assess and formulate recommendations to MFAC on the long-term role of Universities in the fusion program. The related excerpt from Dr. Trivelpiece's letter states:

> "To assist the Department in making effective use of its university-based fusion research program, I would like MFAC to advise us on the long-term role of universities in the fusion program.
>
> Fusion research involves the triad of universities, national laboratories, and industry. Because of the evolutionary nature of the fusion program, the role of each of them will likely change. Therefore, emphasis should be put on exploring new forms of participation, rather than solely changing the level. The Office of Fusion Energy has the responsibility to ensure that these changes strengthen the overall program.
>
> At present, universities are generally involved with smaller scale exploratory research. However, the approach toward reactor conditions tends to imply larger and more costly physics and engineering experiments, as well as more focused and detailed reactor studies. Therefore, these differences must be taken into account as universities review their programs.
>
> Considering the evolving nature of the fusion program, what could the Office of Fusion Energy do, and what could universities do to maintain strong and productive university-based research?
>
> Furthermore, the magnetic fusion energy program, will need a continuing supply of creative, appropriately trained scientists and engineers. A recent manpower study performed for the Office of Fusion Energy indicates that sufficient numbers of technical personnel are likely to be available in the coming decades, for certain specific technical disciplines. However, fusion requires the application of general technical expertise to specific fusion problems as well as certain specialized skills in these areas. What actions could the Office of Fusion Energy take now to be

certain that universities will, in fact, be able to produce the types and quality of personnel needed by the fusion program in the future?

The Energy Research Advisory Board (ERAB) has been asked to examine the broad issue of universities' research and development activities, and to address specifically, the effectiveness of the Department's policies and working relationships with the academic community. A copy of the Deputy Secretary's charge letter to ERAB is enclosed.

I would like your final report on this topic by July 1983."

As discussed at the January 11 meeting of MFAC, preparation of the panel report and recommendations relating to this charge should be carried out within the context of the following general guidelines.

1. The panel should give a clear (and concise) summary of present University-based fusion research in applied plasma physics, confinement systems, and development and technology program areas. The impact of University research programs, student training, etc., on fusion energy development should also be assessed and documented.
2. In formulating recommendations regarding the evolving role of Universities in fusion energy development, be sure to identify research and development activity areas where joint University-Laboratory and University-Industry programs would be advisable.

As a general remark, given the implications and magnitude of the issues, it is very important that panel deliberations be thorough and that recommendations be technically and programmatically sound.

Good luck in this important undertaking. It there is any way I can be of assistance, don't hesitate to contact me.

RCD:mm

xc: Review Panel Assessing the Long-Term Role of Universities
in the Fusion Program
Magnetic Fusion Advisory Committee
MFAC Executive Secretary

MFAC Panel on the Long-Term Role of Universities in the Fusion Program

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MEMORANDUM TO: John Gilleland

FROM: Ron Davidson RCD

SUBJECT: Charge to Review Panel on Programmatic Roles of an Upgraded MFTF-B Facility

DATE: January 11, 1983

Thank you for agreeing to chair the review panel on possible programmatic roles of an upgraded MFTF-B facility.

Enclosed is a copy of the January 10, 1983, letter from Dr. Alvin W. Trivelpiece outlining two areas for evaluation and recommendation by the Magnetic Fusion Advisory Committee (MFAC). The charge to your panel is to formulate recommendations to MFAC on the possible programmatic roles for MFTF-B following the completion of its initial experimental program. The related excerpt from Dr. Trivelpiece's letter states:

> "In my June 1, 1982 letter to the Committee, I requested MFAC to consider programmatic roles for the TFTR, following feasibility experiments. It is also important that the other major facility, MFTF-B, be utilized to the greatest benefit of the fusion program. Therefore, as an extension of my charge #3 to MFAC last June, I would like the Committee to consider possible programmatic roles for MFTF-B following the completion of its initial experimental program.
>
> In your deliberations, you should take into consideration the possibility of technology-oriented roles for MFTF-B, as well as physics-oriented roles. Facilities upgrade modifications should be included in your deliberations, in analogy with the charge on TFTR.
>
> I would like your final report on this topic by April 1983."

As in the evaluation of upgrade options for TFTR, preparation of the panel report and recommendations relating to this charge should be carried out within the context of the following general guidelines.

1. The panel should delineate possible roles of Industry in fabrication and operation of MFTF-B upgrade options.

2. Recommendations and program plans for upgrade options should be developed for two budget cases:

   (a) Flat budget case (in constant dollars), corresponding to \$400 M, \$500 M, or \$600 M for the overall magnetic fusion program in FY84 and beyond.

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(b) Enhanced budget case (but limited to an average real growth of 10% per year, say), corresponding to an accelerated ETR decision point consistent with what is technically feasible.

3. To the maximum extent possible, realistic cost estimates are to be used in formulating plans and recommendations.

With regard to guideline #2, based on the input from the previous MFAC panels, it should be kept in mind that the Magnetic Fusion Advisory Committee will ultimately formulate recommendations to DOE within a total budget envelope for the overall magnetic fusion program.

As a general remark, given the implications and magnitude of the issues, it is very important that panel deliberations be thorough and that recommendations be technically and programmatically sound.

Good luck in this important undertaking. If there is any way I can be of assistance, don't hesitate to contact me.

RCD:mm

xc: Review Panel on Programmatic Roles of Upgraded MFTF-B Facility
Magnetic Fusion Advisory Committee
MFAC Executive Secretary

MFAC Panel Evaluating MFTF-B Upgrade Options

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Ithaca, NY 14853

Mrs. **Bouquard**. Thank you very much for your testimony and for your report as well.

After MFAC gets through integrating and prioritizing its findings from these recent reviews, do you think it might be best to restrain from such activities, your advisory activities long enough to just let DOE just sit down and sift through the findings that you have put together and come up with a good, long-term program? That is, have the DOE really take a good look at what you have done?

Dr. **Davidson**. I would say that it's an ongoing process. The first year that our committee has met, we have met on four occasions so far, we received several charges from the Department. We responded to some of them, and I believe it is probably a correct assessment that our recommendations in certain areas have already had an impact on the departmental planning and prioritization of various elements.

Mrs. **Bouquard**. I'm talking about a long-term program.

Dr. **Davidson**. Are you suggesting that we might be finished in a relatively short time?

Mrs. **Bouquard**. No, that's not what I said at all. I was suggesting a hiatus to look at what we've done and then decide on a plan for a long-term program.

Dr. **Davidson**. We could certainly take a break for a quarter because this has been a tremendous effort on the part of a large number of senior scientists and engineers.

Mrs. **Bouquard**. Sometimes we do that ever once in a while. Look at what we've done and then get back with it. You don't have to do everything in 1 month. I mean the DOE should fully take advantage of what you have done, that is the point I was trying to make.

Dr. **Davidson**. I believe the Department is already taking advantage of what we're doing in the planning process.

Mrs. **Bouquard**. Do you think there will be a change in your priorities if the 1984 budget were raised to the level that DOE has requested? Would this have an impact on your priorities?

Dr. **Davidson**. We looked at three budget cases as I suggested at the beginning. And there is quite a range in those cases.

Mrs. **Bouquard**. Would that have any impact on the priorities that you would set?

Dr. **Davidson**. It certainly would. If you look at the details of the material that was submitted to you then what you can actually do within the various program elements at the different funding levels differs. For example, in the alternate concepts area we concluded at a $400 million level it was not possible to take a major alternate concept step. On the other hand, at a $600 million level it would be possible to take one or two concepts and advance them to their next stage of development. So we've been looking at the details of prioritization within different segments of the program.

Mrs. **Bouquard**. As we all know we've made a lot of advances in the fusion program. Do you reall feel that a level of funding that has been proposed for fiscal year 1984 is going to allow us to capitalize on these advances?

Dr. **Davidson**. One of the findings and recommendations that we made to the Department, which was an area in which our advice certainly was not solicited, commented on just that point. We were very struck in looking through all of the confinement programs, tandem mirrors, tokamaks, and alternate concepts, that during the past 18 months there has been very significant technical progress, which would be consistent with an accelerated fusion program. That is a fusion program funded at a higher level than the fiscal 1984 request.

Mrs. BOUQUARD. Mr. Winn?

Mr. WINN. Thank you, Madam Chairman.

Dr. Davidson, when the advisory committee was evaluating the magnetic fusion energy program, what note, if any, was made to the extent of the industrial and college and university participation? Any contributions in the program?

Dr. DAVIDSON. Certainly in the area of looking at TFTR upgrade options. One of the recommendations that panel 3 made to our advisory committee was that there should be major industrial involvement in the follow-on step to TFTR. With regard to the university programs we have received a charge, in which we have been asked to look at the long-range role of universities in fusion research. Professor Gross at Columbia University is chairing that panel, which meets next week. The panel has representatives from universities, industry, and government that will be involved in formulating recommendations to us.

Mr. WINN. I might say that it's a very impressive panel—impressive advisory council that you put together. Many of those individuals have been before the committee, some several times. We're very impressed with the caliber of the personnel that you have there.

Thank you, Madam Chairman.

Mrs. BOUQUARD. Thank you, Mr. Winn. Thank you, Dr. Davidson. Thank you, Dr. Trivelpiece. Thank you all for being with us this afternoon.

The next witness we would like to welcome is Herbert Woodson from the University of Texas at Austin. He's director for the Center for Energy Studies and Chairman for the Committee on Magnetic Fusion, National Research Council.

Thank you for being with us, Dr. Woodson. We have your testimony. You may proceed as you wish. You may summarize. Your entire testimony will be made part of the record.

STATEMENT OF DR. HERBERT H. WOODSON, DIRECTOR, CENTER FOR ENERGY STUDIES, THE UNIVERSITY OF TEXAS AT AUSTIN, AND CHAIRMAN, COMMITTEE ON MAGNETIC FUSION, NATIONAL RESEARCH COUNCIL

Dr. WOODSON. Thank you very much. Madam Chairman and members of the Science Committee, I want to thank you for this opportunity to discuss the National Research Council study of the engineering needs of fusion.

Before I summarize the findings and conclusions of the study, let me say a little bit about it and how it was carried out.

Mrs. BOUQUARD. Please do.

Dr. WOODSON. The National Research Council is the operating agency of the National Academy of Science and the National Academy of Engineering and is responsible for the conduct of research and study services for the Government, the public, and the scientific and engineering communities. The members of our committee were chosen for their special competences and most were members of the National Academy of Engineering.

Our committee organized a workshop at which about 20 fusion technologists made presentations and our deliberations were strengthened by the participation of a score of senior engineering professionals outside fusion from the computer, automotive, aerospace and utility industries, almost all of whom were also members of the National Academy of Engineering.

The charge to our committee, and I should have prefaced this by saying that this study of ours was actually requested by the Office of Fusion Energy of the Department of Energy. The charge to our committee was: First, how clearly can the future engineering development needs of fusion be specified? and, second, how can the engineering development capacity of industry be effectively coupled to the ongoing scientific development in national laboratories and universities?

In addition to answering those two questions an important objective of the exercise was to stimulate an expanded dialog between knowledgeable fusion technologists and senior engineers from fields involving related technologies such as those I mentioned earlier.

Mrs. **BOUQUARD**. Excuse me, Dr. Woodson. Would you supply for this committee the makeup of your committee?

Dr. **WOODSON**. Yes, ma'am. I believe that each one of you has received a copy of our report. This report has a roster of the committee, it also has a roster of those that were invited to attend, and it has a program of the workshop that we sponsored. [This information is appended to conclusion of Dr. Woodson's oral testimony.]

Mrs. **BOUQUARD**. Thank you very much. We do have this information and it will be entered in the record.

Dr. **WOODSON**. After hearing the presentations and discussions in the workshop and receiving the reactions of the special invitees our committee prepared a report. I must emphasize that this report does not pretend to represent either a comprehensive review or critique of the current fusion program, nor does it present an alternative proposed program. Rather, it is an excerpted and condensed version of thoughts and reactions expressed by people both inside and outside the fusion program, and interpreted by a committee of greatly varied backgrounds and interests. As a consequence, it presents findings and conclusions, but not recommendations. Furthermore, the report is intended for a larger audience than just the fusion community.

As the considerably substantial investigation of magnetic fusion proceeds from scientific experimentation towards practical application, it will encounter engineering challenges of enormous complexity. The future engineering needs of fusion must be well understood to receive proper emphasis in the program conducted by the Department of Energy. The first charge to our committee was to determine how well such future engineering development needs can be identified.

An overall conclusion relevant to the object is that:

> The technology and engineering needs of magnetic fusion can be identified with a relatively high level of confidence within the boundaries of the accumulated scientific evidence and the adopted design objectives. Development programs have been initiated to address many of these needs.

I believe it would be fair for you to consider this statement as "cautiously optimistic." It is optimistic in that we are able to identify the engineering development needs and, in many but not all cases, the facilities and programs needed to fulfill them. But, this optimism should be tempered with caution because fusion science is still advancing and we do not know what changes in our understanding future advances may dictate. In short, although hopes are high and the potential payoff is great, considerably more remains to be done before commercially attractive applications can be identified, proven, engineered, and adopted.

You may wish to refer to my prepared statement submitted for the record or to the report itself which contains a concise summary for more detailed look at each of the 20-plus issues we examined. This afternoon, I want to highlight the more important findings, and elaborate a moment on them.

First, you should remember that the engineering development needs for all magnetic confinement concepts have much in common. The heating and confinement of a reactor-grade plasma require the same physical processes regardless of the approach we take. Moreover, the technological problems associated with handling high particle and heat fluxes, of converting the fusion energy to electricity, and of breeding tritium are also generic ones. A very strong magnetic field is required for confinement; heating can be accomplished by a transfer of large amounts of energy to confined plasma particles; materials must be able to tolerate extremely energetic particles, very high heat fluxes, and neutrons of energies that are unique in our industrial experience; and finally, provision must be made for the breeding and control of tritium for fuel. Conceptual design studies of reactors have been the principal means by which these engineering needs have been identified.

Our ability to confine and heat plasmas is, as you know, orders of magnitude better today than it was as recently as 6 years ago. Whether we rely on superconducting or copper magnet coils, the engineering needs are well defined in terms of field strengths and volumes. However, maintainability and reliability must be proven in a reactor-grade environment. We know how to heat plasmas to conditions that exceed those found on the Sun by factors of 5 to 10. Neutral beam injection, radio frequency heating, and ohmic heating are technologies of enormous value, but each has serious limitations in the engineering sense. In addition, neutral beams and rf systems need substantial technological advancements to reach currently perceived reactor relevance and clear choices for engineering development await further experimentation.

Impurity control and, to a lesser extent, ash removal are recognized as generic needs that pose some of the most difficult problems to be solved in fusion, and their solution is essential to achieving long burn times or steady-state operation. Of the two options to deal with the problem in tokamaks, a choice betweem pumped limiters and magnetic divertors cannot be made now for lack of an adequate data base on reactor-grade plasmas. In each of these instances associated directly with the plasma environment, it will take reactor-prototypical systems of at least intermediate scale and operating under realistic fusion conditions to demonstrate these subsystems fully prior to their incorporation in an experimental reactor.

As we move out beyond the plasma into the other components of the fusion reactor we encounter issues dealing with (a) blanket assemblies, (b) tritium systems, and (c) materials that can be subdivided into those that must survive direct contact with the plasma environment, and those that function as structures, heat sinks, and insulators and are subjected to severe neutron bombardment.

The engineering needs of blanket assemblies are well defined for D–T fueled reactors, but the detailed design is uncertain though, because essential information on material properties is lacking, as is any experience in designing, building, and operating blanket systems.

The development needs for tritium recovery and control systems are well defined, and a special facility for their study exists now in the soon-to-be-operational tritium systems test assembly at Los Alamos.

The first-wall problems resulting from plasma-materials interactions appear to be so severe, unique, and important that they need special attention for their solution. The general nature of these problems is understood, but the directions for specific engineering development are not well characterized.

On the other hand, the duty required of structural materials is well defined, and it appears that adequate performance for near-term [ETR] reactors is likely from available materials. However, the existing data base is insufficient for engineering design of more advanced reactors; and, for advanced alloys that might be used in commercial fusion power reactors, the data base is so sparse that the acceptability of these alloys cannot be established at this time. Before the final design of a power reactor for construction, all these subsystems will have to be proof-tested in a fusion test reactor that produces flux and fluence close to those of actual operating fusion conditions.

Also, a great deal of attention is being paid to the requirements for remote disassembly and remote maintenance. But, until we know more about which components will last the lifetime of the reactor and which parts will have to be periodically replaced, we cannot say with certainty what the final configuration should be.

The program already relies heavily on systems engineers and sophisticated project management techniques because its experiments are so large and complex. Increased emphasis here will be necessary as future machines become even larger and more complex.

I would now like to turn our attention to the second charge of the committee; that of more effectively coupling the capacity of industry to the fusion program. The participation of industry will, in the long run, give form and purpose to the commercial applications of fusion. The time at which fusion power can be commercially attractive and power plants can be marketed is far in the future—probably more than three decades; as a consequence, capital investment at this time in expectation of a reasonable return on that investment is unrealistic. Short of capital investment, however, there are other useful forms of participation. The fusion program needs the perspective and expertise found in industry in several areas—component supply, engineering services, and personnel exchange. Future benefits to the program as well as great readiness of industry can be realized through increased industrial participation in defining requirements for fusion devices, in upgrading them, and in managing projects.

As I noted earlier, a number of senior engineering professionals from fields other than fusion were invited to our workshop both for their reactions and to foster a stonger dialog between workers in the fusion program and the larger engineering community. Their reactions were significiant enough to warrant a chapter in our report. A majority of these engineers concurred that the future engineering development needs of fusion are well on their way to clear definition, and none currently identified appear to be of such a character as to preclude their solution. Specifically, two areas were thought to warrant the greatest need for futher identification and effort; the engineering needs for materials to be used in the severe plasma and nuclear environment of a reactor, and those for the breeding, recovery, and control of tritium.

There was almost unanimous agreement that fusion is important as a potentially plentiful energy source. Nevertheless, views differed widely as to how rapidly fusion can probably be developed, irrespective of whether the pace is limited by technology or by funding. In either case, a long program of research and development is expected, and serious concern was expressed about how to sustain the program at an adequate level of funding for long enough.

The view was expressed that until both scientific and technological development are much more mature, exclusive engineering choices made in the near-term—such as concept selection—would be based on inadequate data and understanding. Accordingly, continued attention to alternative concepts is prudent.

The technology developments sufficient to support experiments, including engineering test devices, are not generally sufficient at this time for engineering design and specification for commercial, or even demonstration, reactors. As a consequence, the fusion program's engineering development activities will need to be expanded eventually to include the full performance testing and qualification of all components for commercial or demonstration fusion applications.

A recurring theme throughout this exercise was the crying need for a realistic fusion environment with which to develop and test materials, components, and systems. It is extremely important to know at the earliest possible time how a seriously fusing plasma actually behaves and how it can be controlled—to possess in our laboratories a real fusion environment with its attendant neutrons, charged particles, and radiation. If we can do that, then we can learn to live with it.

The actual developments proposed to satisfy the engineering needs of fusion require proof and the rigorous environment of a reactor-grade fusing plasma.

A point that we believe important for the Congress and the fusion community to understand and appreciate is that the national fusion program appears to be planned in great detail as an orderly development program should be, but it is one that requires dramatic breakthroughs and inventions that generally come from more freewheeling research programs. This need is implicit throughout the program. But as Hans Bethe has said, and I quote him, "while scientific miracles do happen occasionally in scientific history, it would be dangerous if Government administrators, or even scientists and engineers imagined that they should be expected." There are few things as irretrievable as the right answers to the wrong questions. We must guard against becoming prisoners of preconceived answers. A goal-oriented, milestone-laden research program does not have to and should not sacrifice flexibility.

As the fusion program continues its transition from scientific research to engineering development it is useful to contemplate carefully the stages it must go through. Although they can be characterized in a variety of ways, I prefer the succinct and cogent characterization presented to our committee so eloquently by Dan Simpson, vice president of Westinghouse-Hanford. He described fusion, like any developing technology, as having three natural stages: First, scientific development; second, technology development; and third, systems engineering. The first stage includes the development and proof of theory and scientific feasibility. We are essentially within 2 years of that goal so elusive for three decades. The second stage, technology development, both provides technological support for the scientific development and establishes the basis for subsequent engineering development. This is the stage we are now moving into rapidly. Technology to engineer, construct, and operate successfully the necessary scientific experiments is that type of technology development the fusion community has proven itself very good at. Witness TFTR. Technology development to support the system engineering phase of fusion power such as determining key materials characteristics and properties is our challenge for the balance of the eighties and beyond. System engineering, which includes the demonstration of engineering and economic feasibility, will come later after enough scientific development has been done and the technology development program has generated the needed information. I found it most useful to think in terms of these overlapping stages as I consider the program, its recent progress and its future plans.

The committee should remember that a wholly new science has been created where none existed a few short years ago. Investigators of fusion have made enormous strides in scientific understanding and technology development. These accomplishments are testimony both to their competence and their dedication. The fusion community has demonstrated considerable talent in tackling the unknown and in achieving the difficult.

Labels aside, the fusion effort is nothing less than man's halting but resolute attempt to replicate mother nature at her magnificent best.

From my perspective I must say that, overall, I am optimistic about the future of fusion, and I hope these remarks may be in some way useful to you.

I thank you for inviting me and I'll be happy to answer any questions.

[National Research Council Study personnel and workshop program, and the prepared statement of Dr. Woodson follow:]

APPENDIX A

STATEMENT OF TASK*

A committee will be created to plan and conduct a two-day workshop in Washington, D.C., and to document the results of the workshop.

The committee will solicit assistance from relevant parts of the National Research Council and other organizations in the public and private sectors. The workshop will provide a forum for a dialogue between knowledgeable fusion technologists and senior experienced technologists from fields involving related technology, such as aerospace technology, electronics, robotics, nuclear technology, and electrical power systems.

The workshop has three objectives:

1. Determine how clearly the future engineering development needs for fusion can be specified.
2. Determine how, with an engineering development plan, the engineering development capacity of industry can be effectively coupled to the coincident ongoing scientific development in national laboratories and universities.
3. Prepare a report delineating the findings and discussions of the committee.

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*Excerpted largely from the Notice of Grant Award from the U.S. Department of Energy to the National Academy of Sciences.

APPENDIX B

WORKSHOP PROGRAM

OPENING SESSION

9:00 a.m., August 3, 1982

Objectives of the Workshop
Herbert H. Woodson, University of Texas

Current Strategy in the Fusion Program
John F. Clarke, U.S. Department of Energy

SESSION 1. SYSTEMS DEFINITION

Organizer: Weston M. Stacy, Jr., Georgia Institute of Technology
9:30 a.m., August 3, 1982

Technology and Engineering Needs of Magnetic Fusion as Derived from Near-Term Tokamak Design Studies
D. Steiner, Rensselaer Polytechnic Institute

A Review of Commercial Tokamak Fusion Reactor Conceptual Design Studies
C. C. Baker, Argonne National Laboratory
R. W. Conn, University of California at Los Angeles

The Future Engineering Needs of Mirror Fusion Reactors
K. I. Thomassen, Lawrence Livermore National Laboratory

Identification of Future Engineering Development Needs of Alternative Concepts for Magnetic Fusion Energy
Robert A. Krakowski, Los Alamos National Laboratory

SESSION 2. SUBSYSTEM TECHNOLOGY, I: PLASMA SUBSYSTEMS

Organizers: Melvin B. Gottlieb, Princeton University
Z. John Stekly, Magnetic Corporation of America
1:45 p.m., August 3, 1982

Future Engineering Development Needs for Magnetic Fusion: Superconducting Magnets
D. Bruce Montgomery, Massachusetts Institute of Technology

RF Technology for Magnetic Fusion Reactor Applications
John M. Rawls, General Atomic Company

Status of Impurity Control/Energy Removal Systems for Magnetic Fusion
John A. Schmidt, Princeton Plasma Physics Laboratory

SESSION 3. SUBSYSTEM TECHNOLOGY, II: NUCLEAR SUBSYSTEMS

Organizers: Everett Bloom, Oak Ridge National Laboratory
William D. Manly, Cabot Corporation
4:00 p.m., August 3, 1982

Blanket Development for DT Fusion Reactors
Dale L. Smith, Argonne National Laboratory

Tritium Handling and Recovery Requirements for Fusion Reactor Systems
James L. Anderson, Los Alamos National Laboratory

The Development of Materials for Fusion Power Reactors
R. E. Gold, Westinghouse Electric Corporation

Plasma Material Interaction Technology
Walter Bauer, Sandia National Laboratories

SESSION 4. SYSTEM INTEGRATION

Organizer: John W. Landis, Stone & Webster Engineering Corporation
10:15 a.m., August 4, 1982

Systems Integration and Maintenance Are Important Considerations in the Development of the Tokamak Configuration
Charles A. Flanagan, Oak Ridge National Laboratory

Remote Maintenance and System Integration of Fusion Devices from the Users' Standpoint
Dwain F. Spencer, Electric Power Research Institute

Primary Problems in System Integration, Testing and Construction of Fusion Power Plants
William J. L. Kennedy, Stone & Webster Engineering Corporation

SESSION 5. PARTICIPATION OF INDUSTRY

Organizer: John W. Landis, Stone & Webster Engineering Corporation
2:00 p.m., August 4, 1982

Planning the Program to Fit the Goals
Joseph G. Gavin, Jr., Grumman Corporation

Utility Participation in Magnetic Fusion Programs
Betty K. Jensen and K. L. Matson, Public Service Electric & Gas Company

Magnetic Fusion: Industrial Participation, the Bridge to Ultimate Success
John B. Yasinsky, Westinghouse Electric Corporation

APPENDIX C

WORKSHOP PARTICIPANTS

COMMITTEE ON MAGNETIC FUSION

- HERBERT H. WOODSON (Chairman), Center for Energy Studies, University of Texas
- EVERETT E. BLOOM, Metals and Ceramics Division, Oak Ridge National Laboratory
- MELVIN B. GOTTLIEB, Plasma Physics Laboratory, Princeton University
- JOHN W. LANDIS, Stone & Webster Engineering Corporation
- WILLIAM D. MANLY, Cabot Corporation
- WESTON M. STACEY, JR., School of Nuclear Engineering and Health Physics, Georgia Institute of Technology
- Z. JOHN STEKLY, Magnetic Corporation of America

WORKSHOP SPEAKERS

- JAMES L. ANDERSON, Los Alamos National Laboratory
- CHARLES C. BAKER, Fusion Power Program, Argonne National Laboratory
- WALTER BAUER, Physical Science Department, Sandia National Laboratories
- JOHN F. CLARKE, Office of Energy Research, U.S. Department of Energy
- R. W. CONN, School of Engineering and Applied Science, University of California at Los Angeles
- CHARLES A. FLANAGAN, Fusion Engineering Design Center, Oak Ridge National Laboratory
- JOSEPH G. GAVIN, Jr., Grumman Corporation
- ROBERT E. GOLD, Advanced Reactor Division, Westinghouse Electric Corporation
- BETTY K. JENSEN, Public Service Electric & Gas Company
- WILLIAM J. L. KENNEDY, Stone & Webster Engineering Corporation
- ROBERT A. KRAKOWSKI, Los Alamos National Laboratory
- D. BRUCE MONTGOMERY, Plasma Fusion Center, Massachusetts Institute of Technology
- JOHN M. RAWLS, General Atomic Company
- JOHN A. SCHMIDT, Plasma Physics Laboratory, Princeton University
- DALE L. SMITH, Fusion Power Program, Argonne National Laboratory
- DWAIN F. SPENCER, Advanced Power Systems Division, Electric Power Research Institute
- DONALD STEINER, Nuclear Engineering Department, Rensselaer Polytechnic Institute
- KEITH I. THOMASSEN, Mirror Fusion Test Facility, Lawrence Livermore National Laboratory
- JOHN B. YASINSKY, Advanced Power Systems Division, Westinghouse Electric Corporation

SENIOR ENGINEERING PROFESSIONALS

PAUL F. CHENEA, General Motors Corporation (retired), Birmingham, Michigan
WILLIAM A. FREDERICK, Pennsylvania Power & Light Company
JOHN R. GILLELAND, Doublet III Division, General Atomic Company
MARTIN GOLAND, Southwest Research Institute
WALTER F. LEVERTON, Consultant for Management and Technology, Pacific Palisades, California
CLIFFORD D. MARKS, St. Louis Division, McDonnell Douglas Astronautics Company
JAMES W. MOYER, Physical Systems Research and Development, Southern California Edison Company
BERNARD M. OLIVER, Hewlett-Packard Company
HEINZ G. PFEIFFER, Pennsylvania Power & Light Company
CHARLES E. REED, Bridgeport, Connecticut
DANIEL E. SIMPSON, Engineering and Safety Department, Westinghouse Hanford Company
JOHN W. SIMPSON, Consultant, Hilton Head, South Carolina

TESTIMONY before

HOUSE SCIENCE & TECHNOLOGY COMMITTEE

SUBCOMMITTEE on ENERGY RESEARCH & PRODUCTION

15 MARCH 1983

"THE FUTURE ENGINEERING NEEDS OF MAGNETIC FUSION"

Report of the National Research Council
Committee on Magnetic Fusion

by

- HERBERT H. WOODSON
The University of Texas at Austin

THE FUTURE ENGINEERING NEEDS OF MAGNETIC FUSION

Madam Chairman and members of the Science Committee, thank you for this opportunity to discuss the National Research Council study of the engineering needs of fusion.

Before I summarize the findings and conclusions of the study, let me say a little about the study and how it was carried out. The National Research Council is the operating agency of the National Academy of Science and the National Academy of Engineering, and is responsible for the conduct of research and study services for the government, the public, and the scientific and engineering communities. The members of our committee were chosen for their special competences and most were members of the National Academy of Engineering.

Our committee organized a workshop at which about twenty fusion technologists made presentations and our deliberations were strengthened by the participation of a score of senior engineering professionals outside fusion from the computer, automotive, aerospace and utility industries, almost all of whom are also members of the National Academy of Engineering.

The charge to our committee was to address these questions:

1. How clearly can the future engineering development needs of fusion be specified? and,
2. How can the engineering development capacity of industry be effectively coupled to the ongoing scientific development in national laboratories and universities?

In addition, an important objective of the exercise was to stimulate an expanded dialogue between knowledgeable fusion technologists and senior engineers from fields involving related technologies such as those I mentioned earlier.

After hearing the presentations and discussions in the workshop and receiving the reactions of the special invitees our committee prepared a report. I must emphasize that this report does not pretend to represent either a comprehensive review or critique of the current fusion program, nor does it present an alternative proposed program. Rather, it is an excerpted and condensed version of thoughts and reactions expressed by people both inside and outside the fusion program, and *interpreted* by a committee of greatly varied backgrounds and interests. As a consequence, it presents findings and conclusions, but not recommendations. Furthermore the report is intended for a larger audience than just the fusion community.

As the considerably substantial investigation of magnetic fusion proceeds from scientific experimentation towards practical applications, it will encounter engineering challenges of enormous complexity. The future engineering needs of fusion must be well understood to receive proper emphasis in the program conducted by the Department of Energy. The first charge to our committee was to determine how well such future engineering development needs can be identified.

An overall conclusion relevant to this objective is that:

> *The technology and engineering needs of magnetic fusion can be identified with a relatively high level of confidence within the boundaries of the accumulated scientific evidence and the adopted design objectives. Development programs have been initiated to address many of these needs.*

I believe it would be fair for you to consider this statement as "cautiously optimistic." It is optimistic in that we are able to identify the engineering development needs and, in many but not all cases, the facilities and programs needed to fulfill them. But, this optimism should be tempered with caution because fusion science is still advancing and we do not know what changes in our understanding future advances may dictate. In short, although hopes are high and the potential payoff is great, considerably more remains to be done before commercially attractive applications can be identified, proven, engineered, and adopted.

You may wish to refer to my statement submitted for the record or to the report itself which contains a concise summary for a more detailed look at each of the 20-plus issues we examined. This morning, I want to highlight the more important findings, and elaborate on them a bit.

First, you should remember that the engineering development needs for all magnetic confinement concepts have much in common. The heating and confinement of a reactor-grade plasma require the same physical processes regardless of the approach we take. Moreover, the technological problems associated with handling high particle and heat fluxes, of converting the fusion energy to electricity, and of breeding tritium are also generic ones. A very strong magnetic field is required for confinement; heating can be accomplished by the transfer of large amounts of energy to confined plasma particles; materials must be able to tolerate extremely energetic particles, very high heat fluxes, and neutrons of energies that are unique in our industrial experience; and finally, provision must be made for the breeding and control of tritium for fuel. Conceptual design studies of reactors have been the principal means by which these engineering needs have been identified.

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As we move out beyond the plasma into the other components of the fusion reactor we encounter issues dealing with (a) blanket assemblies, (b) tritium systems, and (c) materials that can be subdivided into those that must survive direct contact with the plasma environment and those that function as structures, heat sinks and insulators and are subjected to severe neutron bombardment.

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The view was expressed that until both scientific and technological development are much more mature, exclusive engineering choices made in the near term--such as concept selection--would be based on inadequate data and understanding. Accordingly, continued attention to alternative concepts is prudent.

The technology developments sufficient to support experiments, including engineering test devices, are not generally sufficient at this time for engineering design and specification for commercial, or even demonstration, reactors. As a consequence, the fusion program's engineering development activities will need to be expanded eventually to include the full performance testing and qualification of all components for commercial or demonstration fusion applications.

A recurring theme throughout this exercise was the crying need for a realistic fusion environment with which to develop and test materials, components, and systems. It is extremely important to know at the earliest possible time how a seriously fusing plasma actually behaves and how it can be controlled--to possess in our laboratories a real fusion environment with its attendant neutrons, charged particles, and radiation. If we can do that, then we can learn to live with it.

The actual developments proposed to satisfy the engineering needs of fusion require proof in the rigorous environment of a reactor-grade, fusing plasma.

A point that we believe important for the Congress and the fusion community to understand and appreciate is that the national fusion program appears to be planned in great detail as an orderly development program should be, but one that requires dramatic breakthroughs and inventions that generally come from more freewheeling research programs. This need is implicit throughout the program; but, as Hans Bethe has said, "while scientific miracles do happen occasionally in scientific history, it would be dangerous if government administrators, or even scientists and engineers, imagined that they should be expected." There are few things as irretrievable as the right answers to the wrong questions. We must guard against becoming prisoners of preconceived answers. A goal-oriented, milestone-laden research program does not have to and should not sacrifice flexibility.

As the fusion program continues its transition from scientific research to engineering development it is useful to contemplate carefully the stages it must go through. Although they can be characterized in a variety of ways, I prefer the succinct and cogent characterization presented to our committee so eloquently by Dan Simpson, vice president of Westinghouse-Hanford. He described fusion, like any developing technology, as having three natural stages: First, scientific development;

second, technology development; and third, systems engineering. The first stage includes the development and proof of theory and scientific feasibility. (We are essentially within two years of that goal so elusive for three decades.) The second stage, technology development, both provides technological support for the scientific development and establishes the basis for subsequent engineering development. This is the stage we are now moving into rapidly. Technology to engineer, construct, and operate successfully the necessary scientific experiments is that type of technology development the fusion community has proven itself very good at. Technology development to support the system engineering phase of fusion power such as determining key materials characteristics and properties is our challenge for the balance of the eighties and beyond. System engineering, which includes the demonstration of engineering (and economic) feasibility, will come later after enough scientific development has been done and the technology development program has generated the needed information. I have found it most useful to think in terms of these overlapping stages as I consider the program, its recent progress and its future plans.

The committee should remember that a wholly new science has been created where none existed a few short years ago. Investigators of fusion have made enormous strides in scientific understanding and technology development. These accomplishments are testimony both to their competence and their dedication. The fusion community has demonstrated considenable talent in tackling the unknown, in achieving the difficult.

Labels aside, the fusion effort is nothing less than man's halting but resolute attempt to replicate Mother Nature at her magnificent best.

From my perspective I must say that, overall, I am optimistic about the future of fusion, and I hope these remarks may be in some way useful to you. Thank you.

Herbert H. Woodson, a native of Texas, received his Bachelor of Science, Master of Science and Doctor of Science degrees, all in Electrical Engineering, from the Massachusetts Institute of Technology. After 15 years on the M.I.T. faculty, he joined the faculty in electrical engineering at The University of Texas at Austin where he now occupies the Ernest H. Cockrell Centennial Chair in Engineering. He served as Chairman of the Department of Electrical Engineering from 1971 to 1981, he has been Director of the Center for Energy Studies since 1974, and he is Director *ad interim* of the newly established Center for Fusion Engineering. Dr. Woodson has done research and published in areas most closely related to energy conversion and electric power systems. For the past ten years he has been working on pulse-power systems for fusion and other applications. He has been a consultant to several companies and he is the coauthor of two textbooks.

Dr. Woodson is a registered professional engineer in Texas and Massachusetts, a Fellow of the Institute of Electrical and Electronics Engineers, Past President of the IEEE Power Engineering Society and a member of the National Academy of Engineering.

Mrs. BOUQUARD. Thank you very much for your fine testimony.

Mr. Winn, would you like to go first?

Mr. WINN. Thank you, Madam Chairman.

Several times during your testimony you referred to the fusion community. I'm not sure I know exactly what that encompasses.

Dr. WOODSON. The fusion community, to me, involves the collection of people who are working on some aspect of the fusion program. This involves the national labs; it involves the programs at the various universities; it involves the programs of companies who are actually involved in programs at their own sites; it also involves those company personnel who are on detached duty at places like Oak Ridge and Princeton. The fusion community is that group of folks who are working on different kinds of fusion problems.

Mr. WINN. How would that vary because a couple of times you referred to the larger engineering community? That's where I'm a little confused.

Dr. WOODSON. The larger engineering community, to me, is simply the community within the country and the world that worries about a lot of other engineering problems besides fusion. These are people that design and build conventional powerplants; these are people that engineer our communication systems——

Mr. WINN. A broad base of——

Dr. DAVIDSON. Yes.

Mr. WINN. That's very helpful.

I'm going to sound like I'm in a rut, but you heard my previous questions. When the National Research Committee gathered material for its recently issued report, what note, if any, was made at that time about the extent of industrial or university or college participation in the fusion energy program?

Dr. DAVIDSON. One of our charges was to see how the development capacity of industry could be coupled better into the program. That was one of our specific charges. Beyond that we didn't really look at the amount of participation from each one of these groups: industry or universities or national labs. We simply didn't address whether or not the most appropriate people are doing the work. We simply looked at the questions we were asked without trying to evaluate the participants beyond making some assessment of the overall quality of the work in the program. That really is determined by the progress that has been made over a number of years.

Mr. WINN. Even though it could be said you had a parochial interest in this; do you have any personal opinions of whether there's good communications; whether the advisory council committee is enough of an outlet for the input or accepts the input of the university and industry people? Do you have any personal opinions?

Dr. WOODSON. Yes. In my——

Mr. WINN. It won't affect the business of the University of Texas.

Dr. WOODSON. Thank you, sir.

I would say that in my perception there are very good lines of communication. I can confirm that Dr. Trivelpiece gets a lot more advice than he really needs and I can confirm that he will listen to almost anyone. I call up and ask may I come and talk to you and he'll let me come and talk with him. As far as I'm concerned, from what I see, there is ample opportunity for communication within the community.

Our National Research Council committee did discuss at length the idea of industrial participation in the program and one of the first questions that one would ask is: Is industry about to invest a lot of capital right now? Invest a lot of money in the expectation of making a profit like they would in their normal course of business. The answer was no. The time at which you would begin to make money is so far in the future it simply is not worth while.

I think our committee also concluded that industry has talents, expertise, experience and all that really are needed by the fusion community. They are availing themselves of this in many cases, and we encourage them to do this where it is appropriate. I don't think we went into detail to see whether or not there was more they should do or less they should do but we certainly encouraged them to go find it and use it wherever it is.

Mr. **Winn**. You covered that pretty well in your testimony; talking about the management capability of industry. That was mentioned in the earlier conversation with Dr. Trivelpiece.

I think you've answered my questions. Thank you very much. You're a good witness.

Thank you, Madam Chairman.

Mrs. **Bouquard**. Thank you, Mr. Winn.

I'd like for you to comment on the relationship between our labs and universities in our fusion engineering program. Do you think we are really operating at the optimum? Where do you think improvements could be made?

Dr. **Woodson**. I haven't really studied that particular interaction in great detail so I think I'm probably not prepared to give you a carefully thought-out answer. In our experience we have very good relations with the national labs. They certainly welcome interactions with us. I don't perceive that there's a great problem, from my point of view. I heard Dr. Schoet yesterday and I'm simply not cognizant of the situations that he was describing. I'm not in a position to comment on those comments.

Mrs. **Bouquard**. Do you think that our fusion development could be enhanced by higher funding level or do you think we're being limited by technology that you outlined in your statement?

Dr. **Woodson**. From what I perceive, the fusion program could spend effectively a larger amount of money than it's getting now. I think it has the capability to make good use of additional funds if it got the additional funds.

Whether or not it should get additional funds is a question that I don't really have an answer to because that is a question of relative budget priorities among different categories within the Department of Energy and the national budget. That's something that I haven't studied or our committee didn't study and I don't think we are prepared to comment on. I will say that I think there is the capability there to use more money effectively if it were available and I think that a fraction of that would go to the universities. As Dr. Trivelpiece said, there is a substantial amount of money already being spent in the universities, and I think this would continue to be so. Yes, I think they could and additional university support would be effective.

Mrs. **Bouquard**. Let's get out here in the dream world and say you have no financial restraints. We have all the money we want and we can do whatever we want to do. How rapidly do you think fusion could be developed?

Dr. **Woodson**. As an engineer who has spent 25 years, mostly in the electric power industry, I'm afraid if you had an unlimited budget you couldn't possibly see a commercial fusion reactor in operation in less than 50 years.

Mrs. **Bouquard**. Do you share the belief of the Department of Energy that in 1988 we're going to be able to select between the tokamak and the mirror?

Dr. **Woodson**. I think that by 1988 it would be possible to make that particular choice. I think it was the conclusion of our National Research Council Committee that a choice made at that time might well be premature with respect to the ultimate long-range choice that would be better for the future of fusion.

I think clearly by then, assuming that TFTR performs as expected and assuming that MFT-B is finished and performs as expected, there will be additional scientific data on the basis of which one could make a choice between tokamaks and mirrors.

That would simply eliminate the alternate concepts from consideration in the future deliberations—maybe they are going to compete further down the line with the one that won in 1988. I think that a choice between those two concepts in 1988, in terms of the overall fusion program, will be premature. I think it should come later. Personal opinion.

Mrs. **Bouquard**. First of all you would not recommend choosing between the two in 1988?

Dr. **Woodson**. That is right.

Mrs. **Bouquard**. In your testimony you referred to the conceptual designs studies as really a principal means to identify our engineering development for our reactors. Do you think that our current activities in this area are sufficient?

Dr. **Woodson**. I personally think so. I think it was the consensus of our committee that this is so. We have had some very detailed reactor design studies done. We are continuing to refine those designs in considerable detail. I think at the current state of development of fusion, the detail one should go to should be limited to the point of identifying the problem that you really need to get after. In my opinion, the reactor designs that have been done so far have been of sufficient detail to satisfy the need. I think as time goes by, we need to continue to update those reactor designs in the light of any new scientific or technological information. But I think our level of effort there is adequate.

Mrs. **Bouquard**. Thank you very much. You're an excellent witness. Thank you for being with us today.

I want to thank all of you for being here and all of you that have participated in our audience. Thank you for being with us.

The next meeting of this committee will be our budget markup next Tuesday at 1:30 in the Energy and Commerce room.

There is no further business. The committee stands adjourned.

[Whereupon, at 3:49 p.m. the committee was adjourned until Tuesday, March 22, 1983.]

APPENDIX

Atomic Industrial Forum, Inc.
7101 Wisconsin Avenue
Bethesda, Maryland 20814
Telephone (301) 654-9260
TWX 7108249602 ATOMIC FOR DC

May 3, 1983

Ms. Julie Shisler
Subcommittee on Energy Research
and Production
B374 Rayburn House Office Building
Washington, D.C. 20515

Dear Ms. Shisler:

Enclosure (1) is submitted in response to Mrs. Lloyd's request of April 12, 1983 to supplement the subcommittee hearing record on the FY 1984 DOE budget request for the Magnetic Fusion Energy program.

Sincerely,

Kenneth L. Matson

Kenneth L. Matson
Chairman, Policy Panel
Committee on Fusion

KLM:jl

1 Enclosure

- Staff question - You recommended a $20 million increase in the Department of Energy's FY 1984 proposed budget for Magnetic Fusion Energy. Aside from the $1 million that you recommended be earmarked for development in industry of an engineering development plan, how would you distribute the other $19 million add-on?

In my prepared testimony I noted that the Fiscal Year 1984 Department of Energy proposed budget for Magnetic Fusion Energy increases the program budget by $20 million over the FY 83 budget. We have not analyzed how the $20 million should be applied to the program recognizing that the $20 million represents an increase of only 4.5%, roughly maintaining pace with inflation. The important point is that the $20 million increase represents the minimum necessary to continue to move the program forward. Anything less would mean some retrenchment that would further limit the program's progress.

- Staff question - You recommended setting aside annual funding for a number of industrial initiatives in cooperation with selected, national laboratories and universities to develop needed engineering data. What types of initiatives do you have in mind, and what level of funding is recommended?

This recommendation would permit industrial initiatives in areas where industry has experience and knowledge such as environment and safety studies, maintenance developments, commercial reactor requirements, etc. In such a program the expertise of industry, including direct electric utility involvement, would be integrated with selected national laboratories or universities in a jointly developed work/expenditure statement. Such efforts would promote cooperation between industry and the national laboratories or universities and begin engineering tasks these groups believe are important to the demonstration of engineering feasibility. The funding for such a program would be modest. An initial funding of $2 million in FY 84 would provide a meaningful beginning for a program of industry initiatives.

Department of Energy
Washington, D.C. 20585

April 29, 1983

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy Research
and Production
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Ms. Lloyd:

On March 17, 1983, Dr. Alvin W. Trivelpiece appeared before the Science and Technology Subcommittee on Energy Research and Production to discuss the FY 1984 budget request for the magnetic fusion energy program.

Following that hearing, you submitted written questions to supplement the record. Enclosed are the answers to those questions.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

for Thomas W Pretorius
Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

We have been continuously assured by you and the Office of Fusion Energy for months that the Comprehensive Program Management Plan is to be delivered any day now. What is the status of the Plan?

The Plan has just been put into the formal process of concurrence leading to ultimate approval by the Secretary. At the beginning of this year, we had initiated such a formal consideration but found that we had not adequately addressed all of the concerns of those in this review process. I believe that our reexamination of the draft and clarification of a number of points have resolved these concerns. Consequently, I expect that the formal concurrence and approval process should be conducted and concluded within a few weeks. Although the Plan is obviously late, I believe that the long interval between initial development and the current draft has been used profitably to strengthen the quality of the Plan's substance.

The Department originally asked OMB for $505 million for FY 1984, and received $467 million instead -- a cut of $38 million. What specific programs were reduced and by how much were they reduced?

The OMB reduced the Department's request by $38.0 million. We chose to allocate this reduction, based on our judgment of program priorities, in the following areas; $15.5 million from the Elmo Bumpy Torus Proof-of-Principle project, $5.0 million from the Mirror Fusion Test Facility - B project, $7.0 million from design studies for an upgrade to the Tokamak Fusion Test Reactor device, $4.0 million from the Development and Technology subprogram, $3.5 million from the Elmo Bumpy Torus-S activity, $2.0 million from the Applied Plasma Physics subprogram and $1.0 million from the tandem mirror confinement systems activity. Some difficult decisions had to be made, but we are left with a balanced program at the proposed funding level.

As regards the status of EBT-P funding, when is the Department going to come forward with a proposal for spending the $10 million provided by the continuing resolution? What do you intend to do with these funds and other EBT-P funds unexpended to date?

The Department will have a detailed plan in the near future for EBT-P. As you are aware, technical reviews were conducted of the EBT Program and EBT-S data. The Panels concluded that new EBT-S experimental results suggest a reexamination of the data base, confinement time, and scaling analysis. Based on this information the Department is reevaluating its position on EBT-P.

How can we be sure that the findings of the EBT-P review panel can be adequately addressed with the current EBT-S device? Aren't we running the risk of funding research on a device which may not really be able to address the important technical issues of EBT-type devices?

The physics questions which precipitated the recent reviews of the EBT program were first raised by the Oak Ridge National Laboratory physicists working on the EBT-S experiment. The EBT-S measurements indicated that previous scaling assumptions, which had been used to design the EBT-P device, were now less certain. As a result, the design of a next-step device is more risky. The EBT Senior Review Panel recommended that the EBT program give top priority over the next 12 to 18 months to raising the temperature and confinement time values in EBT-S, and improving our basic understanding. The panel further recommended that any future steps in the EBT program be decided on the basis of the outcome of this effort, and that the design of the EBT-P be kept flexible while these issues are pending. We concur with these judgments.

Your testimony stresses that materials are a critical element in the fusion program, and that facilities for the production of intense neutron fluxes are required. If international funding does not become available for FMIT, do you plan on proceeding anyway, or will the facility be cancelled?

We have no plans to proceed with FMIT on our own if international funding does not become available. If international collaboration on FMIT does not materialize, then a review of the overall fusion materials program world-wide would likely be conducted to determine whether any satisfactory, though later, action to meet program needs can be found.

When are polarized fuel experiments expected to take place? How soon can we expect results which will either confirm or negate the theoretical predictions?

A scientific workshop was held at the University of Wisconsin on March 28-30, 1983, to examine the potential of polarized fuel reactions for application to the fusion power development program. The theorists who originated the idea were participants as were about forty other scientists with expertise in the topical areas that would be needed to create and use polarized fuels. The results of this workshop have not yet been published but personnel from the Office of Fusion Energy who did attend formed some preliminary viewpoints.

Integrated experiments on mainline fusion experiments in which a polarized fuel interacts at fusion producing temperatures will not be possible for a number of years. Development of sources of adequate capacity to fuel such an experiment would probably require substantial development time regardless of which of several source options might be used.

However, a more pertinent issue is whether or not the scientific promise of increased fusion reactivity from polarized fuels would be likely to produce worthwhile benefits to a power producing fusion reactor.

The Office of Fusion Energy is gathering information from a number of informed sources and, during the summer, expects to identify the practical possibilities that could be reasonably developed in this area.

Your testimony indicates that the Magnetic Fusion Energy Program's computing requirements continue to grow. The Department has presented this Subcommittee with detailed data indicating that the MFE Computer Network is vastly oversubscribed. This being the case, what is the basis for assigning 800 hours to other users outside the program? How much funding is the program being reimbursed to provide these 800 hours? Might it not have been appropriate to notify the Subcommittee of MFE's plan to take such a step?

The Office of Energy Research funds forefront research in High Energy and Nuclear Physics, Basic Energy Sciences, Health and Environmental Research, and Magnetic Fusion. The scientists and engineers in those programs have access to some of the most advanced experimental facilities in the world but, with the exception of Magnetic Fusion, they do not have access to the modern computers required for large-scale scientific calculations. There is a class of problems in energy research which requires a large scale computer capability. Thus the Office of Energy Research has decided to provide 800 hours of Cray-1 time per year on the Magnetic Fusion Energy Network to other programs in the Office of Energy Research. This represents about 5% of the total Cray-1 time available to users.

The MFE network is not oversubscribing in the sense that there is more time allocated to users than there is available. At the present time, it is my judgement that providing 5% of the MFE computer time to attach a few of the most important problems in the other Energy Research programs may have a higher potential payoff than using that time for the residual 5% of the fusion program's allocation. It is our intention to seek a permanent solution to the ER computing problem in the near future.

The fusion program will be reimbursed for the computer time allocated to other ER programs at a rate of about $950 per hour. Since it is planned to begin allocating this time in May 1983, this will amount to about $300,000 in FY 1983 and about $760,000 in FY 1984.

DON FUQUA (Fla.), Chairman

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Executive Director
ROBERT C. KETCHAM
General Counsel
DAVID S. JEFFERY
Minority Staff Director

April 12, 1983

Dr. Alvin W. Trivelpiece, Director
Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

Thank you for your testimony at our Subcommittee hearing on the FY 1984 DOE budget request for the Magnetic Fusion Energy program.

We would like to supplement the record with your response to the attached questions on the program. It would be most helpful if we could receive your response on or before May 5, 1983. Please forward them to the attention of Ms. Julie Shisler, Subcommittee on Energy Research and Production, B374 Rayburn House Office Building, Washington, D.C. 20515.

Should you have any questions regarding this matter, please contact Dr. John V. Dugan, Jr., Subcommittee Staff Director, at 202/225-2884.

Thank you for your cooperation.

Sincerely,

Marilyn Lloyd

MARILYN LLOYD, Chairman
Subcommittee on Energy
Research and Production

ML:S
Enclosure

ADDITIONAL QUESTIONS

Dr. Trivelpiece

- We have been continuously assured by you and and the Office of Fusion Energy for months that the Comprehensive Program Management Plan is to be delivered any day now. What is the status of the Plan?

- The Department originally asked OMB for $505 million for FY 1984, and received $467 million instead -- a cut of $38 million. What specific programs were reduced and by how much were they reduced?

- As regards the status of EPT-P funding, when is the Department going to come forward with a proposal for spending the $10 million provided by the continuing resolution? What do you intend to do with these funds and other EBT-P funds unexpended to date?

- How can we be sure that the findings of the EBT-P review panel can be adequately addressed with the current EBT-S device? Aren't we running the risk of funding research on a device which may not really be able to address the important technical issues of EBT-type devices?

- Your testimony stresses that materials are a critical element in the fusion program, and that facilities for the production of intense neutron fluxes are required. If international funding does not become available for FMIT, do you plan on proceeding anyway, or will the facility be cancelled?

- When are polarized fuel experiments expected to take place? How soon can we expect results which will either confirm or negate the theoretical predictions?

- Your testimony indicates that the Magnetic Fusion Energy Program's computing requirements continue to grow. The Department has presented this Subcommittee with detailed data indicating that the MFE Computer Network is vastly oversubscribed. This being the case, what is the basis for assigning 800 hours to other users outside the program? How much funding is the program being reimbursed to provide these 800 hours? Might it not have been appropriate to notify the Subcommittee of MFE's plan to take such a step?